

Karl Pearson and the Galtonian tradition: studies in the rise of
quantitative social biology.

Submitted for degree of Ph.D.

by

Bernard John Norton.

100
100
100

B.Norton, Karl Pearson and the Galtonian tradition.

ABSTRACT

This work discusses the growth of a 'Galtonian tradition' in Science, notably as developed by Karl Pearson and his colleagues. It traces the development from Galton's ideas, in Britain, of the disciplines of Statistics, Biometrical Genetics and the Psychology of individual differences. These developments were linked with a number of philosophical and ideological commitments on the parts of the scientists concerned, and the work examines the interplay between these commitments and the theorising of the scientists. It looks also at the relations that may have held between these commitments and the social milieu of the scientists. Particular attention is given to the role of the strong and influential Eugenics movement which flourished in Britain at the time of these developments.

CONTENTS

Chapter 1. Introduction	1
Notes	14
Chapter 2. Francis Galton, founder of the Galtonian tradition.	15
Notes	57
Chapter 3. The Biometric School and the Profession of Statistics.	63
Notes	112
Chapter 4. Karl Pearson and Statistics. The cultural relations of scientific innovation	117
Notes	150
Chapter 5. The Mendelian-Biometrician controversy. Problems in the History of Genetics.	153
Appendix 1. The rationale for Weldon's opposition to Mendelism	190
Appendix 2. The Esher letter	194
Notes	195
Chapter 6. The rise and fall of the law of ancestral heredity.	201
Notes	221
Chapter 7. Eugenics as a popular movement, would-be science and midwife of science. The case of R.A. Fisher	223
Notes	259
Chapter 8. The growth of Galtonian Psychology. Intelligence objectively defined and measured.	264
Appendix The sampling theory	288
Notes	291

Chapter 9. Conclusions	295
Notes	302
Select Bibliography.	303.

Acknowledgements

In preparing these chapters more debts have been incurred than can ever be repaid. In particular I would like to offer thanks to A.D.Birnbaum, W.F.Bynum and E.S.Pearson, all of whom gave without counting the cost.

I would also like to acknowledge financial assistance from the Social Science Research Council and the Wellcome Foundation.

CHAPTER I

INTRODUCTION.

- (a) Some rationales
- (b) Some issues in explanation
- (c) Some background developments
- (d) Contents

(a) Some rationales

The nineteenth century is distinguished by its biologically minded philosophers. One thinks immediately of Chambers, Darwin, Haeckel, Spencer or Huxley. Yet, in truth, few of these can rank in consequence with Darwin's cousin, Francis Galton (1822-1911). He was the founder of a whole Galtonian tradition of Darwinian social biology and psychology, which, in its exfoliation, has given rise to several multi-million dollar knowledge industries.

Galton conducted his research mathematically, and set in train a series of intellectual and institutional developments eventuating in the modern discipline and profession of statistics - more precisely, of mathematical statistics. This is now widely pursued and taught either as an end in itself, as a methodology capable of rendering the social sciences truly scientific, or as a technology capable of improving the productivity of agriculture and industry.

Because he desired to know the differences between men, and between classes and races of men in respect of their mental constitutions, Galton undertook work that laid the foundation for a strong 'individual difference' tradition in psychology. Its development has resulted in a series of statistical theories of intelligence and in the cognate social technology of mental testing. Testing requires testers, and they need money. It is illuminating to note that by 1954 the Carnegie Foundation alone^I had allocated over six million dollars to mental testing.

Being a Darwinian thinker, Galton investigated heredity in man. Thereby he founded a tradition of mathematical biology which, over time, and via several controversies, has developed into the modern discipline of biometric or quantitative genetics. This has notable applications in agriculture and in the controversy as to whether observed differences between races in performance on standardised tests are of mainly genetical or of mainly environmental origins. There is a direct link, as we shall see, between the thought of Galton and the genetics of Sir Ronald Fisher, and another direct link between these and the work of Arthur Jensen.

Here, then, we have the development of three disciplines - statistics, mathematical genetics and the psychology of individual differences, now all elevated to the status of major knowledge industry. They all stem in good degree from the thought of Galton, from that of his greatest follower, Karl Pearson (1857-1936), and, of course, from the thought of men who interacted with them. These included men such as William Bateson the geneticist, Ronald Fisher the geneticist and statistician, Charles Spearman the author of the notion of a central factor of 'intelligence', and Sirs Cyril Burt and Godfrey Thomson the distinguished mental testers. The essay that follows examines the development of the various disciplines from Galton's and Pearson's thought, not in a 'whiggish' fashion, but in a manner that follows historical dead ends as well as the historical paths of glory.

So far, I hope to have established the notion that Galton was influential, in the sense that his ideas were taken up and developed by many others into academic disciplines and cognate technologies. And, it might be argued that the fact that this pattern of development has remained unstudied is sufficient reason for embarking upon such a study. But, the reasons for looking at the Galtonian tradition go deeper than its unstudied condition and manifest influences. For, its development offers a first class case-study of scientific growth which can illuminate the roles of human values, of ideology and of metaphysics in the production and consumption of scientific theories and of associated technologies. The tradition, we shall see, was organically connected with philosophical and ideological views. The essay that follows, then, examines the human well springs of scientific endeavour in some particular cases.

Galton himself, and many of his followers - including Pearson, Burt and Fisher - were committed eugenists. That is to say, they believed that alcoholism, pauperism, mental defect and many other conditions were due predominantly to inherited dispositions or 'diatheses', and that improvement and advance would be best achieved by differential breeding from those sections of the community whom they judged to be the 'fit'. They promoted the view that class differentials in fertility were bringing about national decay via genetic erosion. Eugenics, as we shall see, was generally closely linked to another creed of the age - that of 'social Darwinism' which employed Darwin's theory to obtain 'natural' guides to morality and social policy.

Social Darwinism was not the only form of 'extra-scientific' commitment found among the developers of the Galtonian tradition. Karl Pearson, for example, held to an extreme positivism and intended his statistical biology to be an exemplar of his philosophy of science - making him one of the few philosophers of science ever to attempt to practice what he preached. Charles Spearman was also heavily involved in, and motivated by philosophical and social considerations. In the work following I shall trace the strong interactions between the development of the Galtonian tradition and these and other philosophical and ideological strands.

So far, it might be thought that the development of the Galtonian tradition was an exclusively British phenomenon. This is not the case. In America too there was a strong pattern of activity. As in Britain, there were 'Galtonian' statisticians, 'Galtonian' geneticists and 'Galtonian' psychologists. Above all, there was a eugenics movement whose influence quite outstripped that of its British counterpart. While the British Eugenics Education Society was still discussing the legality of voluntary sterilisation, American states had gone ahead and passed a veritable spate of compulsory sterilisation laws. By January 1, 1958, there had been some 31,038 sterilisations carried out, of which² no fewer than 7,518 had been conducted in California, then, as now, in the van of change. It was America too which saw the early flowering of mass mental testing - in the form of the Army alpha tests, which assigned first world war recruits to privatehood or to officerhood, to potato peeling or to intelligence work.³

By now the picture should seem large, if sketchy. In Britain and in America (and in Europe too), patterns of work growing out of Galton's ideas led to important developments. It is not that every statistician, psychologist or geneticist of note can be seen as in any strong sense following Galton's programme. But a goodly number of important ones can. And, several of the others can be seen either as reacting against the tradition or as working from institutional bases established by adherents of the Galtonian tradition. Lancelot Hogben is an example of the first sort, George Udny Yule an example of the second.

The whole canvas cannot be covered in detail in a reasonably lengthy work. A focus is required. Here, it lies squarely with the seminal contributions of Karl Pearson and his 'Biometric School'

of biologically orientated statisticians which developed in London in the last decade of the nineteenth century, and in the first decades of the twentieth. Pearson's school played a crucial role in transforming Galton's ideas into a series of powerful scientific movements. Roughly speaking, it was Pearson and his school which developed Galton's ideas while creating a new would-be science of biometry - or, mathematical evolutionary biology. Out of biometry came forth statistical ideas and institutions, including the world's first ever statistics department in an anglophone university, the journal Biometrika, a good deal of mathematical genetics and a methodology which enabled interested psychologists to follow through and develop certain of Galton's ideas in a quantitative fashion. Pearson is frequently regarded as the founder of the modern discipline of statistics. The Biometric School under Pearson was also heavily involved in eugenics, which it attempted to establish as the social science par excellence. In 1925, Pearson set up the Annals of eugenics, which still survives, and, indeed, flourishes, as the Annals of human genetics. In short, Pearson and his school are the key figures in the development of the Galtonian tradition.

(b) Some issues in explanation.

In preceding paragraphs, I have suggested that the Galtonian tradition was developed by men with strong political and philosophical beliefs. And, in what follows, I will examine with some care the ways in which these 'extra-scientific' beliefs affected the course of scientific development. The effect, I will suggest, was a very considerable one. Both the perception of problems as problems, and the nature of attempts made to resolve them, it will be argued, can frequently be understood only by understanding the philosophical and ideological condition of the minds doing the perceiving and the resolving. There is, I consider, absolutely nothing in such a claim that modern philosophers of science would find implausible. Men like the late Imre Lakatos have repeatedly stressed that science develops within research programmes which, over time, refine, expand and generally articulate some strongly held initial hypothesis beloved of the scientist in charge of the programme. Lakatos and others have also stressed that the process is one that requires an active, imaginative mind.⁴ The initial hypotheses have to be actively conceived, for they cannot fall out of the data in the manner once imagined by the more witless of Bacon's followers. Quite possibly therefore, we shall find that the scientist's style of creativity harmonises with, reflects, and, indeed, may be explained by, his more general 'extra-scientific' values. To suggest this is not to

suggest a relapse into relativism. For the initial creation and the subsequent attempted development of hypotheses are two different things. Whether the realities of nature will allow any particular development to go ahead is a matter for empirical test. A cheese-monger may hypothesise that the Moon is made of gorgonzola, but the reports of astronauts may kill the hypothesis. Of course, few interesting hypotheses stand in such a simple relation to potentially falsifying observations as that, but, most philosophers would agree, the fate of an hypothesis is not independent of some external reality.

But, supposing for the moment that a man's value may affect his science, then how are we to explain his having these values in the first instance? Do they come to a man by accident, or, are they determined by his social position? How shall we explain them?

Clearly, values are not rigidly determined by social position. Only one wounded Spanish nobleman became a Loyola. Some millionaires live like tramps. If we looked for rigid determinism in human affairs, we would be defeated at every turn. But, we know, explanation is possible and the basic tack taken here will be roughly as follows. Groups of men within society tend to adopt ideologies and desired social blueprints within which they play esteemed and valued roles. Thus, for example, a proletarian may propose the dictatorship of the proletariat as a source of sweetness and light; a professional man may perceive the virtues of a state run by him and his fellows according to some suitable professional ideal, and an aristocrat will perhaps extoll the virtues of a hierarchical civilisation in which the rapacious manufacturer is restrained within his proper place by the exercise of feudal powers. We cannot hope to predict what the members of a group will recommend, because, generally, there are several possible social scenarios in which the group may discern an elevated role, and, of course, because many groups and individuals are so worn down by prevalent power structures that they are unable or unwilling to entertain such speculations. But, the group's recommendations once known, we can sometimes see its social role as an important determinant of what was recommended. It is much like the case of a man who develops a disease. We can discern that his exposure to mumps virus is the cause of his distended cheeks without supposing that everyone so exposed will so wax. We cannot, of course, prove that the explanation given is correct, but it may perhaps be defended if attacked. In this work, several examples of this strategy will be developed. At least on occasions, I shall suggest that a particular scientist's values may be ^{partially} explained by exhibition as the common and

appropriate values of the group to which he belongs. There are many severe difficulties associated with such a strategy, and it is perhaps better to deal with them as they arrive than to offer an essay on historical methodology at this point.

(c) Some background developments

This study deals with scientists who, for the most part, were located within the social constructions of Victorian and Edwardian England. And, if the sorts of explanation just mentioned are to feature in the study, it may be well to include, within this introduction, an indication of some major changes in social structure during this period.

A development of great significance, as Donald MacKenzie and others have pointed out, is the emergence of a strong and powerful professional middle class, distinguished from the working class, the entrepreneurial middle class and the aristocracy. This development has been frequently remarked upon, notably in Professor Perkin's Origins of modern English society, where⁵ the author discusses in some detail the rise of an independent professional class, in the mid and late Victorian period, emancipated from dependence upon the aristocracy by increased demand created in turn by the rise of incomes and by the urbanisation produced in the industrial revolution. These new men, described by Hobsbawm as occupying a 'nouvelle couche sociale', with their 'comparative aloofness from the struggle for income', increased in numbers and power during the nineteenth century, and, as Perkin has noted, were the authors of various social blueprints. At first, he argues, 'professional men moralized the ideals of other classes by transforming them from mere apologetics for self interest into moral theories of society', but, with the passage of time, he holds, 'the professional ideal became uppermost in the minds of the professional thinkers, and increasingly alienated their adopted class'. Many of the men studied here fall into this group, or identified with it, and could see their science as advancing the ideals of the group.

From the 1830's to the 1870's, Britain was taken as closely as any society ever has to a condition of complete laissez faire in economic matters.⁶ And, with notable exceptions, the intellectuals among the professional middle classes given to thinking about such things seemed happy with the arrangement, discerning within it all sorts of advantages. The early utilitarians could ally themselves with an entrepreneurial class

against 'idle' property and 'corrupt' patronage, as exemplified by the aristocracy. But, as Hobsbawm and others have noted, the conditions - social, military and industrial - which underpinned an ideology of atomistic individualism began to crumble in the 60's and 70's, and there emerged onto the scene new groups of middle class ideologues stressing the need for collectivism of one sort or another and emphasising the role which the cultivated expert, the professional man himself, would play within a new and reformed order. Matthew Arnold's Culture and anarchy, providing 'inspiration for a system of universal education' came out in 1869. And, in short order we have the British positivists under Harrison, the Hegelian liberalism of T.H. Green, the professional 'socialism' of the Fabians and a new breed of social imperialists, willing and anxious to contribute to the 'national efficiency' movement which was such a prominent feature of the first years of the twentieth century, in which fear of growing Japanese and German competition was mobilised to support social change in the direction of a more collectivist, a more 'efficient' state. How much actual social reform can be laid at the feet of, say, the Fabians or the pupils of T.H. Green or Bosanquet is an issue unwise to address lightly, especially as the amount of paper produced by a group gives no accurate guide to the power it exercised. But, what is important, is that, with the century's ageing, men could argue for collectivist measures of one sort or another without being thought stupid, dangerous or crazy.

This shift was paralleled in the field of what has become loosely known as 'social Darwinism'. Poor Darwin, one feels, would have turned in his grave had he been acquainted with this creation, particularly with some of the turns it was to take at the end of the century. But, there was a long-standing tradition of melding biological and social thought. The first 'social Darwinist', of course, was Herbert Spencer, who spoke of the 'survival of the fittest' some years before that phrase's adoption by Darwin. Spencer was the popular philosopher of mid-Victorian times, and offered guarantees of unstoppable human progress in the form of an integrated or 'synthetic' system of biosocial philosophy. Improvement in society would come about by adaptation to social conditions, and the new habits of mind which this adaptation engendered within the survivors would be transmitted by inheritance to a following generation, which would start the cycle of adaptation and transmission once again - leading ever onwards and upwards, so to speak. This adaptation was to be facilitated by a social policy of laissez faire, of letting the individual rub up against

the reality of his or her existence. Spencer's philosophy was, generally speaking, one of hope, and involved the extinction of the less fit only if they failed to make appropriate adaptations. The great thing, biologically speaking, was that these adaptations were supposed inheritable, and the analogy of nature led Spencer to doctrines which, in practice, came close to demanding laissez faire as a biological necessity - the oldest and crudest form of social Darwinism. But, with the passage of time, this form of Darwinism, though beloved of the great captains of American industry,⁷ tended to give way to another. Towards the end of the century, and in parallel with the shift from social atomism to varieties of collectivism and national efficiency, there arose a new 'external' form of social Darwinism. This, we shall see, emphasised struggle too, but the struggle featured was a different one. Progress, in this new perspective, was the consequent not of the struggle of everyman with his neighbour, but of the struggle of group with group. The analogy of nature was now seen to demand the analogue of a well organised herd. Such a herd, with rational division of labour would win in inevitable group struggle, especially against groups permitting damaging intestine strife. Evidence for these remarks will be brought forward where appropriate, but, for the moment, a flavour of the shift may be garnered⁸ from Sidney Webb's tract in the Fabian essays of 1889. We find him arguing that,

we know now that in natural selection at the stage of development where the existence of civilised man is at stake, the units selected are not individuals but societies.

It is against these trends in British society that the events discussed in the remainder of the work were played out. At each stage I shall attempt to spell out these events in greater and adequate detail. We will see that the scientific advances which the work studies cannot be understood in isolation from these trends and changes.

(d) Content.

In the body of the study, the order of procedure will be as follows.

Chapter 2. The first substantive chapter analyses Galton himself. It depicts him as a man whose time finally came, because in his old age, British society was at last prepared to receive his doctrines. These were inappropriate when first expounded in the 1860's. He is shown as a man who entered science as a consequence of a new ideology, which

he hoped to articulate and spread via science. For, in the late 50's, like so many other Victorians, he threw over Christianity and constructed for himself a powerful though intellectually shaky, Darwinian weltbild. The new social order discerned within this order was the eugenic order - an order in which progress would be produced by controlled human heredity, with the control in the hands of the 'men of science' whom Galton so loved to study. It was an order unattractive to the individualism of mid-Victorian Britain, but, by the end of the century and beyond, Galton was able to attract a following for his eugenic ideals - or, at least, for reinterpreted versions thereof. It was in the hope of providing a scientific basis for such an eugenic order that Galton's scientific work - in heredity, statistics and psychology was performed.

Chapter 3. The argument of this chapter contends that Pearson and his school acted as midwife and nurse to the Galtonian tradition. They took in hand Galton's ideas and developed them, institutionalised them and made them available for consumption by others. Galton's statistical ideas were greatly advanced in the course of Pearson's attempt to create a new science of mathematical evolutionary biology (or biometry as it was known) in the last decade of the 19th and the first decade of the 20th centuries. It was Pearson's development of these ideas that enabled, for example, the rapid development of the statistical study of individual differences by psychologists in the early 20th century. And, it was Pearson who did most to institutionalise eugenics as an academic discipline. Again, it was Pearson's ideas that set the starting point for Sir Ronald Fisher's influential work in genetics.

Now, much as the first substantive chapter seeks to explain the production and reception of Galton's work, so the student of the biometrical school must wish to explain why it rose when it did, and to such effect. This task is taken up in the third substantive chapter (ie, chapter 4). The third chapter paves the way, giving a descriptive 'outer' history of the biometric school. It describes Pearson's early work with Weldon, and the way in which this led onto a science of biometry, at first undifferentiated from mathematical statistics, and centred about the journal Biometrika, founded in 1901. It discusses the relations between biometry, statistics and eugenics, outlines the famous 'biometric-Mendelian debate' of the early 20th century, in which Pearson was decisively defeated by the new Mendelian geneticists in a struggle for hegemony in evolutionary biology, and analyses also the not-quite-so-well-known members of the biometric school - men like G.U. Y^uale, W.S. Gossett and Major Greenwood.

The aim of this chapter, in brief, is to discuss the development of Pearson's biometric school, and ^{in particular} to give an account of the ways in which it led on to the formation of a new discipline and profession of statistics.

Chapter 4. This chapter offers an explanatory or 'inner' history of some of the events described in the previous chapter. It looks at Pearson and his school, but, particularly at Pearson - the central and dominating figure. It asks why he should have taken up mathematical biology, though he was no biologist, and, why he should have done this in a manner productive of statistics. The answer is given in terms of Pearson's traverse through late Victorian society, in which he adopted philosophical and ideological ideas which, in the context of the period, could make the doing of a certain type of mathematical Darwinism seem a very attractive proposition. It was just the sort of mathematical Darwinism that would lead to statistical methods being developed, and just the sort of Darwinism that led on to a strong interest in eugenics. We shall see why, in the early 20th century, Galton was prepared to donate, and Pearson prepared to receive, very large sums of money to foster the development of eugenics as an academic discipline.

Chapter 5. This chapter tries to give an explanatory analysis of the famous 'biometric-Mendelian' debate mentioned above. It explains why the opposing sides - a London group led by Pearson and a Cambridge group led by Bateson - should have continued for so long in total opposition, instead of coming to the obvious compromise, namely that the two approaches were complementary, the one dealing with phenotypes, the other with genotypes.

In the chapter I am critical of extant attempts made to analyse and understand the debate, which, increasingly, is becoming a standard object of historiographical discussion, and, of course, provide what I hope is a superior approach.

Chapter 6. In the aftermath of chapter 5, I have thought it strategic to include a discussion of the growth, waxing and ultimate deflation of the famous 'law of ancestral heredity', which, for many years, was the basis of the biometric school's central claim to biological fame and success. It was a law which essayed to predict the, say, probable height or intelligence of a man on a basis of a knowledge of the heights or intelligences of his parents, grandparents, great grandparents, and so on. The chapter discusses the various reformulations which the law went through, from its first introduction by Galton in the 1880's to the time

By this stage then, I shall have examined Galton's ideas and the various ways in which they gave rise to major developments in statistics and biology. Two other substantive chapters deal with the other main pillars of the Galtonian edifice - eugenics and psychology.

Chapter 7 Here, eugenics is put under the microscope, and I argue that while, in Britain, it was something of a flop, both as a popular movement for social reform and as a would-be academic discipline, it did have a stimulating effect on science - particularly upon genetics and psychology. Psychology being the preserve of the eighth chapter, I here discuss the impact of eugenic ideology upon Mendelian genetics, finding reasons for supposing that it was a desire to resolve eugenic problems that led R.A. Fisher to undertake the work which underpins the modern notion of heritability. This argument comes at the close of a chapter which examines and explains the rise of a popular eugenics movement in the years prior to the first war, and which also analyses and reviews attempts made, by Pearson and others, to create a 'science' of eugenics, aimed at demonstrating, amongst other things, that heredity in general was enormously more influential than environment in human affairs, and that shifts in fertility patterns made it likely that a rapid decline in national intelligence, by genetic erosion, was taking place.

Chapter 8. Here I examine the development of Galton's ideas into a tradition of statistical 'individual differences' psychology, via the taking up into psychology of statistical, or, as they were then known, 'biometrical' methods. Workers here discussed include William Brown, Godfrey Thomson and Cyril Burt, but the main focus is upon Charles Spearman and the role he played within the developing studies of human intelligence that were pioneered in the first couple of decades of the present century. Between 1900 and 1910 significant theoretical moves were made, and, by 1918, the associated technology of mass mental testing was being widely applied.

Few theoretical aspects of these developments outrank Charles Spearman's production and promotion of the doctrine of 'g' - the supposed general factor in intelligence. The doctrine has led to Spearman's being compared with John Dalton, and had done sterling service in accounting for what it is that intelligence tests really measure. The chapter addresses the why and the how of Spearman's production and promotion of his conception, and does this by locating his thought within a framework of philosophical and idealogical concern - including eugenics - of which some were general to the period, and others specific to Spearman.

Chapter 9

This discusses the foregoing, and ~~reviews~~ the roles of values, or, more generally, of non-empirical propositions within the scientific process. In particular it analyses the possibility of giving a sociological explanation of the rise of the Galtonian tradition, seeking to locate its exponents at some point between the two extremes of (a) 'disinterested' searchers after 'pure' knowledge, and (b), distinguished soldiers in the British Class War.

The extent to which the chapters are original varies considerably, with the first two substantive chapters showing the smallest amount of originality. But, what is, and what is not thought to be novel is, I hope, always clearly indicated. One important word of caution should be added, namely that the work that follows discusses the contributions made to various disciplines by men and women, who, in senses to be clarified, partook in a Galtonian tradition. From this it should not be inferred that these were the only persons involved in these disciplines. Notes in the text and footnotes should make this clear.

NOTES.

- 1 See C.Karier, 'Testing for order and control in the corporate liberal state', Ed.theory, 22 (1972), 154-176.
- 2 See S.P.Davies, The mentally retarded in society, New York (1959) See chapter 6, 'The social indictment'. See also K.Ludmerer, Genetics in American society: A historical appraisal, London (1972) See chapter 2 to 5.
- 3 See e.g., L.Kamin, The science and politics of I.Q., Penguin books (1977). See chapter 1.
- 4 See I.Lakatos, 'Falsification and the methodology of scientific research programmes' in eds. I.Lakatos and A Musgrave, Criticism and the growth of knowledge, Cambridge (1977).
- 5 See H.Perkin, The origins of modern English society, London (1972). See especially chapter 7.
- 6 See, for example, E.Hobsbawm, Industry and empire, Penguin books (1974). See p.238.
- 7 See, for example, R.Hofstadter, Social Darwinism in American thought Boston (1955) See especially chapter 2 'The vogue of Spencer'.
- 8 In ed.G.B.Shaw, Fabian essays in socialism, London (1889).

Chapter 2. Francis Galton, founder of the
Galtonian tradition.

- (a) Introduction
- (b) Early career.
- (c) Forging a new perspective
- (d) Interpretations
- (e) Social biology and statistics
- (f) The physiology of inheritance
- (g) Psychology
- (h) Institutions

(a) Introduction

The founder of the Galtonian tradition was Francis Galton (1822-1911). As has been suggested, and as will be shown, the development of the tradition hinged crucially upon recruiting Karl Pearson (1857-1936) to his cause - or, perhaps, to a different but similar cause. Pearson found that Galton's work¹

first freed me from the prejudice that sound mathematics could only be applied to natural phenomena under the category of causation. Here for the first time was a possibility - I will not say a certainty of reaching knowledge - as valid as physical knowledge was then thought to be - in the field of living forms and above all in the field of human conduct.

Now, the literature on Galton is large, repetitive and growing. The chapter analyses the main planks of the Galtonian tradition as developed by Galton, and explains how Galton came to his views. Some aspects of the analysis and explanation are traditional, others are novel. We shall have to look at Galton's thoughts on statistics, human biology, psychometrics and social policy.

(b) Early career

Galton was a late developer. The career for which he is famous did not commence before his thirties and it is useful to make first contact with him aged 27, when he was approaching the end of what Pearson has called the 'fallow years'. In that year the popular 'scientific' movement known as phrenology² was on the decline, but, nevertheless, still lingering on. Roughly, phrenology asserted a correlation between head shape and brain shape, and between brain shape and personality in a broad sense. At root lay doctrines of cerebral localisation first expounded by Gall and Spurzheim. Those who nowadays might visit a career guidance specialist could then call upon the services of the phrenologist, a sort of primitive mental tester, for diagnosis and advice. Galton, presumably disturbed by his showing in life hitherto, visited the London Phrenological Institute and had a phrenological analysis drawn up by the phrenologist Donovan.³ This came in two parts: (i), a printed sheet

recording the degree of development attained in many faculties - amateness, adhesiveness, caution, time, tune, comparison, causality and so on - and, (ii), a hand written report with the following peroration.

Men so organised do not attach themselves to literary pursuits from choice; nor do they distinguish themselves in universities. They love bodily exercises and need sports too much to permit them to devote themselves to philosophy and literature. But if circumstances call upon them to work at such matters they can do so, with tolerable success - though it costs them a good deal of effort to keep pace with good men. In a work - as scholars, they are not 'fast men' though they are by no means incapacitated from taking respectable positions, if they will work hard - if they resolve to succeed. Their firmness and self esteem work well for them, when they are enlisted on the right side. As regards the learned professions I do not think the gentleman is fond enough of the midnight lamp to like them, or to work hard if engaged in one of them. To me he seems best fitted for the army, in which I think he would do well. For he is a fairly good observer^c is practical in his turn of mind - rather than speculative; and has, altogether, a good working intellect. His verbal memory is quite strong enough and he has enough of imagination and initiative to help him upwards and forwards.

The report was perceptive enough, and one likes to suppose that it had some effect. In the following year, with the good wishes of the Geographical Society, Galton took an expedition to South West Africa. This was the age when the 'dark continent' was opened. The expedition was successful, and Galton was ushered a new life by his success. It gained for him an entrée into Victorian scientific circles. Thereafter Galton perceived himself as a 'man of science'.

He had been born near Birmingham in 1822, son of S.T. Galton (1783-1844) and F.A. Violetta Darwin (1783-1832). The father was the son of Samuel Galton F.R.S., (1753-1832). The mother was the daughter of Erasmus Darwin, F.R.S., Erasmus and Samuel were both members of the Lunar Society of Birmingham, which also numbered Joseph Priestley and Matthew Boulton among its members.⁴ The family money on the paternal side came from armaments manufacture, and later on, from banking. The Darwins too were well off. Erasmus was a successful physician, and his son Robert, the father of Charles Darwin (who was Galton's cousin) also made a great deal of money.

Samuel was a quaker, but was disowned by fellow quakers for manufacturing

the instruments of war. S.T. Galton was an Anglican.

S.T. Galton was keen to propel Francis into a profession, to get him into 'an occupation useful to yourself and to others', and, to this end, sent him to the King Edward's School Birmingham, and then in 1838, to the Birmingham General Hospital, where Francis was accepted as a house pupil. His school life appears to have been one long round of japes, thrashings and other punishments. His hospital life appears to have been more congenial, though Galton was never able to resign himself to the terrible cruelty of surgical practice at that period.

In 1838, Galton transferred to King's College London for further training in anatomy, physiology and chemistry, and, in the following year, went to Cambridge to take a mathematics degree - a ^{recognised} practice for aspirant doctors in those days before the introduction of the Natural Sciences Tripos in 1851. Galton was a Trinity man, though he seems to have had little respect for its master, Whewell.

Just how hard he worked at his mathematics is hard to say. Letters to his father indicate that he worked at full steam, but there is also evidence that Galton was at least ordinarily fond of social life and the bottle. We know him to have been a fashionable dresser, ^{and} among literary remains of this Cambridge period is an ode to milk punch, lamenting over indulgence and hang-over. Possibly Pearson is right when he suggests that Galton tried to burn both ends of the candle too brightly. In the event the mathematics ^{was} the one that ^{was} extinguished, and, after suffering a mental breakdown, Galton opted instead for a pass degree. Like his cousin Charles Darwin before him, he went out in the poll.

We know little of Galton's Cambridge days, apart from the facts that⁵ he was friendly with the future Sir Henry Maine, with Henry Hallam, brother of Tennyson's Arthur Hallam, with F. Campbell and several others of some note. There is not much documentary evidence ^{to suggest} that Galton was actively involved in religious or political controversy in Cambridge, apart from a couple of minor vignettes. The first concerns Galton's 1843 entry for the Camden medal. This took the form of a poem which, as he put it, was 'relative to the present great controversy as to whether man has a conscience (innate I mean) or not'. It was a long poem, but included the following lines:⁶

Well may we loathe this world of sin, and strain
 As an imprisoned dove to flee away;
 Well may we burn to be as citizens
 Of some state, modelled after Plato's scheme,
 And overruled by Christianity,
 Where justice, love^{and} truth and holiness
 Should be the moving principle of all,
 And God acknowledged as its prop and stay.
 How foolish and how wicked seems the world,
 With all its energies bent to amass
 Wealth, fame or knowledge.

The second vignette is political. Galton, whilst an undergraduate, found himself in a railway compartment along with a member of the Reform Club, a 'ne plus ultra radical'. And, to his family he wrote that,⁷

We had a red-hot argument on politics, which I firmly believe neither of us knew anything about but he would talk about them, and as I must answer yes or no, even Bessy will excuse my not assenting to a radical's ideas.

After taking his degree, Galton returned briefly to his medical studies, and came very close to becoming qualified to practice. But, in 1844 his father died. Galton inherited money and became free from his father's desire that he should become a self-supporting professional man. He quit medicine in an instant, with no regrets.

Thereafter came the 'fallow years'. In 1845-6 he travelled in Egypt, Soudan and Syria, returning to Britain in November 1846. His tours were adventurous but unscientific, though they broadened his mind towards religion. One particular incident is generally noted, Galton and his colleagues, after a debauch, met with a sheikh, fresh from prayer in the desert. He recalled that he 'felt swinish in the presence of his moslem purity and imposing mien'. This, Pearson suggested, is an indication of Galton's coming to think that the Christian faith in which he had been reared was not the only possibility.

On his return to Britain, Galton took to the life of the hunting, shooting and fishing country gentleman. Pearson his biographer clearly found his involvement hard to comprehend.⁸

the strange thing is that it seemed to absorb his whole nature, and to be done not for the sake of^{his} experience, but in the pure pursuit of occupation.

1849-50 was something of a watershed, Galton visited Donovan in the London Phrenological Institute, and, through his cousin Douglas Galton, gained an introduction to the Geographical Society. After discussions, he set up an expedition to Africa - at a time when^{as noted} the opening of the dark continent was a major British interest - and set out for South West Africa in 1850. This kept him out of England until 1852, and his expedition was described by him in his Tropical South Africa.⁹ For his exploits, Galton received the gold medal of the Geographical Society. By turn he threw himself into the affairs of the society, thereby gaining an entrée into the British scientific establishment of the period. Geography led him naturally into meteorology. In 1856 he was made fellow of the Royal Society for his contributions to science, and by then he was three years married to Louisa Butler, a member of Britain's 'intellectual aristocracy'. Galton, of course, had plenty of spare time for science, as he was able to live the life of the rentier, much like his cousin Darwin.

It was not just the Geographical and the Royal societies which received Galton's attention. For, he soon became a great supporter of the British Association for the Advancement of Science, an organisation in which he was to hold high office. He was a natural 'man of science', being, in fact, the one that Beatrice Webb singled out as the finest example of the genre. She saw him as more outstanding than his colleagues Huxley, Tyndall, Hooker and Lubbock.¹⁰

to a recent and enthusiastic convert to the scientific method, the most relevant of Galton's many gifts was the unique contribution of three separate and distinct processes of the intellect; a continuous curiosity about, and rapid apprehension of individual facts, whether common or uncommon; the faculty for ingenious trains of reasoning; and, more admirable than any of these, because the talent was wholly beyond my reach, the capacity for correcting and verifying his own hypotheses, by the statistical handling of masses of data, whether collected by himself, or supplied by other students of the problem.

For Galton, the emerging man of science, 1859 was something of an annus mirabilis. This was not because it was the year of Mill's On liberty or because it was the year of Marx's Critique of political economy, but because it was the time for Darwin's Origin of species.

Galton's interest in the book was anthropological. Like the listeners to T.H. Huxley's addresses he was interested in Darwin for what he might say about man and society. For Galton, a traditional picture of man's place in nature could not be set aside until the Origin: after the Origin, it could not be sustained. In particular, it freed him from the 'argument from design', whereby a long line of British scientists had argued for the existence of a beneficent God. The leading exponent of this tradition at Galton's time was William Paley, whose works all Cambridge students were compulsorily familiarised with.¹¹ That he was glad to escape from Paley's logic cannot be doubted, given a letter written to Darwin, in which Galton thanked his cousin for liberating him from an 'intolerable burden of superstition',¹²

I used to be wretched under the weight of the old fashioned arguments from design of which I felt, though I was unable to prove myself, the worthlessness. Consequently the appearance of your Origin of species formed a real crisis in my life; your book drove away the constraint of my old superstition as if it had been a nightmare and was the first to give me freedom of thought.

Shortly after reading Darwin's Origin, Galton, who had made a study of the races he had encountered in Africa, joined the Ethnological Society, a descendent of the Aborigines' Protection Society set up in 1837 to guard the interests of 'primitive' peoples.¹³ The Ethnological Society was an academic branch of the parent and was directed in the first instance by men like James Cowles Prichard, (1786 - 1848) and Thomas Hodgkin (1799 - 1866) who were much concerned with whether man had had a single origin (the monogenist position) or a multiple origin (the polygenist position). At the time of Galton's joining, the secretary of the Ethnological Society was James Hunt, a strong racialist somewhat in the tradition of the anatomist Robert Knox. Hunt, like other members of the Ethnological Society, held for example, that negroes and white men constituted different species, though he believed in divine creation rather than in Darwinian emergence of species. In 1863, along with some other members, Hunt seceded from the Ethnological Society and founded the Anthropological Society, which could soon count on a fairly diverse membership, including, for example, Captain ^{Richard} Burton. Its membership resolved to 'study man in all his leading aspects, physical, mental and historical', but, the 'Darwinian' faction, including Galton, Lubbock, Huxley and Busk stayed behind and dominated the Ethnological Society. After Hunt's death in 1869, the two groups were reunited, forming, in 1871, the Anthropological Institute of Great Britain and Ireland. Galton thereby

became associated with the official centre of British anthropology, and was to serve as president of the Institute.

So, by the early sixties, Galton had visited Africa, had lost his Christian faith, had encountered the radical racialism of James Hunt and the advanced Darwinian ideas of T.H. Huxley, and, moreover, judging from his writings and correspondence, seems to have *developed* a philanthropic turn of mind.¹⁴ As commentators have noted he wanted to be of some use in the world. We know too that during his African expedition he had formed a low opinion of the negro, comparing the members of one tribe unfavourably with his spaniel as regards mental capacities.¹⁵ He was a ready subscriber to doctrines of black inferiority which constantly recur in *modern* history - whether it be Linnaeus designating¹⁶ the negro as phlegmatic, cunning, lazy, lustful, careless and governed by caprice, or the Anthropological review writing in 1866 that,¹⁷

As the type of the negro is foetal, so that of the Mongol is infantile. And in strict accordance with this we find that their government, literature and art are infantile also. They are beardless children whose life is a task and whose chief virtue consists in unquestioning obedience.

(c) Forging a new perspective

Somehow, out of this melange of observations and perspectives -- strengthened by his having noticed that Britain was developing an apparently hereditary 'intellectual aristocracy' (Galton belonged to two branches of this, the Butlers by marriage and the Darwins) - Galton came forth with a new world view which, as Ruth Cowan and others have noted, replaced his old Christian standpoint and offered him a way to be of use in the world. This was his new 'eugenic' weltanschauung^u which he proposed for the first time in his paper on 'Hereditary talent and character' in 1865.

This paper was, at root, the application of Galton's reading of Darwin's Origin to produce a new account of the order of things. One of the key ideas of the new order, perhaps the key idea, appears in the first paragraph of the paper.¹⁸

The power of man over animal life, in producing whatever varieties of form he pleases is enormously great. It would seem as though the physical structure of future generations was almost as plastic as clay, under the control of the breeder's will. It is my desire to show more pointedly than - so far as I am aware - has been attempted before, that mental qualities are equally under control.

Here we have it. Man himself is subject to selection, which is capable of altering not just his physical nature, but also his psychical nature. Consequently,¹⁹

of the present day
Men and woman/are to those we might hope to bring into existence what the pariah dogs of the streets of an Eastern town are to our own highly bred varieties.

If this were all that Galton wishes to assert, it would be interesting enough. But his claims were wider. He also argued that a variety of social phenomena of first rate importance could be given a Darwinian explanation. Items like parental and social affection, he argued, were explicable naturalistically, because it was obvious that groups of men developing these characteristics would cohere and prosper better than groups that did not.²⁰ Religious sentiments, he argued, were no more than a transmuted form of these evolutionarily founded social sentiments, and a feeling of original sin was no more than a consciousness of recent barbarity. All of this was a good example of the sort of perspective that reading Darwin was liable to produce in Victorian men of science - not in all of them of course, but in that pugnacious fraction including men like Tyndall and Clifford, who actively opposed the force of science to the force of religion. Galton's most interesting doctrine, however, was a statement of what we now refer to as Weismann's principle of the immortality of the germ plasm, which asserts that there can be no inheritance of acquired characters. In this paper Galton threw suspicion on supposed cases of the inheritance of acquired characters, and, after arguing that 'there is nothing in the embryo of an individual that was not in the embryos of its parents',²¹ went on to claim that

We shall therefore take an approximately correct view of the origin of our life, if we consider our own embryos to have sprung immediately from those embryos whence our parents were developed, and these from the embryos of their parents, and so on forever. We should in this way look on the nature of mankind, and perhaps on that of the whole animated creation, as one continuous system, ever pushing out new branches in all directions, that variously interlace, and that bud into separate lives at every point of interlacement.

In short, Galton had a whole new view of things as early on as 1865. The old Christian cosmology was abandoned, and, in its place he substituted a thoroughly naturalistic view, which explained away items such as religious

sentiments in a 'Darwinian' manner. The whole picture of man, which insisted that the individual was merely the product of the exfoliation of the potentialities built into his embryo, was a clean break from the old picture. In the new view, the power of environment was minimal, leaving genetic selection as the only source of human improvement. It is totally different from ^{Herbert} Spencer's view (see below) which allowed that heredity could pass on the adaptive modifications made during one generation's becoming adapted to its conditions. Some evidence for mental heredity was produced in the 1865 paper, but not a great deal - and, certainly, nothing that would force anyone to change his or her mind. Having rejected any divine source for moral guidance, Galton was obliged to look elsewhere - and found the source that many another agnostic or atheistic Victorian was to alight upon, namely biological historicism. He argued, implicitly in 1865, and explicitly later on, that one could discern a necessary tendency of the Cosmos.²² This was the production of 'more and more fit animals'. What else could morality be except the paving ^{of} the way, in kindly fashion, for changes that nature would effect anyway? Natural selection, Galton felt sure, would, in the long run at least, do with great suffering what planned human breeding of humans could do painlessly, or, at least, relatively painlessly. Therefore, planned human breeding as a policy could be defended.

But, the argumentation was not as bloodless as that. Galton might be putting away religion, and replacing it with a natural system of morality, but he did so in a very religious way. He was, after all, a colleague of the aggressive Tyndall, whose Belfast address to the British Association in 1874, usually taken as the classic statement of the anti-religious case, gave a wholehearted acknowledgement of the existence of the religious instinct and argued that providing the sentiment with satisfaction was 'the problem of problems at the present hour'.²³ Galton was a close friend of Tyndall, and so, perhaps, it is not a matter for surprise that he chose to depict the policy of planned human breeding, or 'eugenics' as he called it from 1883 onwards, as a new, secular religion. Naturally, it was to be a religion guarded by a new scientific priesthood. Galton, like some of his fellow leading Victorian men of science, wished to replace the existing Anglican clergy with a new scientific clerisy,

and it is clear from his writing that an important aspect of the work of this clerisy within the scientific state of the future (as planned by Galton) would be the superintendence of a new state religion of eugenics.

The religious feeling which he developed for eugenics is clearly laid out in the closing paragraph of his (Galton's) Inquiries into human faculty of 1883, where he wrote the following,²⁴

The chief result of these inquiries has been to elicit the religious significance of the doctrine of evolution. It suggests an alteration in our mental attitude, and imposes a new moral duty. The new mental attitude is one of a great sense of moral freedom, responsibility and opportunity; the new duty which is supposed to be exercised concurrently with, and not in opposition to the old ones upon which the social fabric depends, is an endeavour to further evolution, especially that of the human race.

Here then we have Galton in full voice. He fervently desired a new, post-Christian source of morals. For him, as for many other Victorians - e.g., in their different ways, Spencer, the brilliant W.K. Clifford and Clifford's successor Karl Pearson - evolution would fill the bill. A non-negotiable view of nature was used to produce a non-negotiable set of moral principles. Nothing would shift Galton from his outlook, not even his friend Huxley's inveighing^{against} this line of biological historicism in his famous address on 'Evolution and ethics' in 1893.²⁵ This is the sort of metaphysics which underlies Galton's work, and that of a great deal of his followers.

In its full form, the Galtonian perspective could take on almost mystic qualities. We find, for example, that Hereditary genius closes on the following note.²⁶

Nature teems with latent life, which man has large powers of evoking under the forms and to the extent which he desires. We must not permit ourselves to consider each human or other personality as something supernaturally added to the stock of nature, but rather as a segregation of what already existed, under a new shape, and as a regular consequence of previous conditions. Neither must we be misled by the word "individuality", because it appears from the many facts and arguments in this book, that our personalities are not so independent as our self-consciousness leads us to believe. We may look upon each individual as something not wholly detached from its parent source, - as a wave that has been lifted and shaped by normal conditions in an unknown,

illimitable ocean. There is decidedly a solidarity as well as a separateness in all human, and probably in all lives whatsoever; and this consideration goes far, as I think, to establish an opinion that the constitution of the living Universe is a pure theism, and that its form of activity is what may be described as co-operative. It points to the conclusion that all life is single in its essence, but various, ever varying, and inter-active in its manifestations, and that men and all other living animals are active workers and sharers in a vastly more extended system of cosmic action than any of ourselves, much less of them, can possibly comprehend. It also suggests that they may contribute, more or less unconsciously, to the manifestation of a far higher life than our own, somewhat as - I do not propose to push the metaphor too far - the individual cells of one of the more complex animals contribute to the manifestation of its higher order of personality.

(d) Interpretations

What are we to make of all of this - of the new post-Christian weltanschauung of Galton, with its thoroughgoing evolutionism, its determination to explain all human sentiments in evolutionary terms, its insistence on the like heredity of mental and physical characters, its 'pure theism', and above all, its insistence on the religious imperatives of eugenics - of the improvement of the human race by selective breeding?

Certainly, the new view was totally different to that which had been adopted by Victorian England's favourite philosopher, Herbert Spencer (1820-1903). Spencer, in numerous tracts and books, had argued that laissez-faire social policies obliged men ^{to} adapt their social habits and mental powers to the circumstances of their lives. By the inheritance of acquired characters, the lessons 'learned' by one generation in the process of improving their adaptation could be transmitted to the next generation, which, accordingly, began its progress from a new and higher base-line.²⁷ Galton's views were directly opposed. There was, for him, no inheritance of acquired characters, and it was state intervention in eugenics, not laissez-faire that would soonest lead to progress. Of course, in the sixties, Spencer's views fitted well with anti-collectivist perspectives then dominant in social thought.

Evolutionism, perhaps, was a reasonably common option for Victorian men of science opposed to the Christian tradition in which they had been raised, and against which they saw the possibility of opposing a 'scientific' picture of things. Galton was restive in his Christianity, but experienced

the strong religious instinct or tendency to which Tyndall alluded. The new view did service in this area of Galton's mental life - enabling him to be both religious and non-Christian at the same time. The religious element should be clear enough in some of the extracts quoted above. At the same time, the adoption of the doctrine of mental heredity gave Galton a practical policy for human reform. It gave his new religion a practical side, combining so to speak, a doctrine of predestination (one was what one's genetic makeup determined one to be) with scope for good works (having children in good numbers if one was talented). Galton, interestingly, was childless.

Ruth Cowan, discussing the doctrine of mental heredity,²⁸ points out that Galton's early commitment to the doctrine exceeded that which was justified by the slim evidence he had been able to amass in the sixties and seventies. Furthermore she points out with perfect justification that the doctrine of the non-inheritance of acquired characters was quite out of step with the biological views of the period. Darwin and Spencer, for example, were firm believers in the inherited effects of use and disuse - roughly, that a blacksmith might transmit strong arms to his offspring, even though the strength of his arms had been achieved by continual exercise rather than by direct heredity.

In order to explain this devotion to nature rather than to nurture, Cowan suggests that we might do well to look to Galton's conservative nature²⁹ (he was a firm supporter of Lord Salisbury) and to his attested desire to be a philanthropist, to be of 'some use in the world'. Certainly, the adoption of the new view of mental heredity enabled Galton to combine a posture of philanthropic reform with a continuing anti-egalitarianism. The social ideal for which he worked was one in which meritocratic ideas loomed large. He wrote for example, that his Utopia³⁰ would be one in which

incomes were chiefly derived from professional sources, and not much through inheritance; where every lad had a chance of showing his abilities and, if highly gifted, was enabled to achieve a first-class education and entrance into professional life, by the liberal help of the exhibitions and scholarships which he had gained in his early youth; where marriage was held in high honour as in ancient Jewish times, where the pride of race was encouraged (of course I do not refer to the nonsensical sentiment of the present day, that goes under that name), where the weak could find a welcome and a refuge in celibate monasteries or sisterhoods, and lastly, where the better sort of emigrants and refugees from other lands were invited and welcomed, and their descendants naturalised.

Given that Galton was so grossly out of step with the doctrines of the times when he first propagated his views in the sixties and seventies, this line of approach seems very helpful. For, whenever we find a scientist holding views which are at variance with established views, we should, presumably expect to find some underlying reason leading him either to (i) suppose that he might be correct and that further work will show this to be the case, or (ii), to actually suppose that his case has been proved. Galton seems to have fallen into the second category, to have gone beyond what available evidence would support at the time. If we think about why this should have been the case, we can see that his total perspective contained many features agreeable to a man constituted as he was - that is to say, one who was disillusioned with Christianity but still of religious disposition, and, at the same time, philanthropically but anti-egalitarianly minded.

(e) Social biology and statistics

Let us now turn to the scientific career which these background beliefs generated. Though personally convinced of the correctness of his views from the outset, Galton desired always to develop them and to produce evidence capable of convincing others. These desires led to seminal work in quantitative social biology, in psychology and in the field of building up an institutional framework to support his ideas.

We may start with quantitative social biology.

After the papers for Macmillan's, Galton went on to produce Hereditary genius in 1869. This work, ~~indifferently~~ received, extended an aspect of the 1865 labours by providing further biographical evidence for the proposition that talent was inherited. As a work it is chiefly notable for its introduction of the normal or Gaussian curve to describe the distribution of human abilities. Galton assumed with ease that 'natural ability', a sort of precursor of the modern notion of intelligence, followed such a distribution. After all, he reasoned, were not several physical characteristics such as height so distributed, and was not this pattern obtained among certain sets of examination marks? (See Fig I and T1 & I)

The first point, if not the second, had been remarked upon by the Belgian astronomer and statistician, Quetelet (1796-1874), who had shown that several human attributes, considered over the whole population,

After the papers for Macmillan's, Galton went on to produce Hereditary genius in 1869. This work extended an aspect of the production of 1865 by providing further biographical evidence ^{from the view} that talent was strictly hereditary. Interestingly, it was not well received: ³¹the Times (Jan 7, 1870) wrote that the proposition that 'very high abilities are very seldom destroyed in the germ is a proposition contrary to all analogy', and the Morning Post (April 16, 1870) wrote that Galton's statistics 'fail altogether in attempting to confirm the continuous descent of genius'. With hindsight, we should say of Hereditary genius that it is ^{most} notable for its employment of the normal or Gaussian curve to describe the distribution of human abilities. Galton assumed that 'natural ability', a sort of precursor of the I.Q. notion, followed such a distribution. After all, he reasoned, were not several physical characteristics such as height so distributed, and was this not the pattern that obtained among sets of examination marks? (See Fig.1 and Fig.2)

The first point, if not the second, had been remarked upon by the Belgian astronomer and statistician Quetelet (1796-1874), who had shown that several human attributes, considered over the whole population,

showed an approximately normal distribution.³² Galton simply appropriated this view and applied it to mental characters. Apparently he^{also} convinced himself that what could be shown for one normally distributed character would hold for any other. Accordingly, we find that his subsequent work frequently did not deal with the facts of mental inheritance directly. Rather, he sought to establish his^{eugenic} case by showing what were the principles governing the inheritance of physical characters such as height, and by arguing that what held for the physical would assuredly hold for the mental too. The reason for this tactic was the simple one that mental measurement was an undeveloped line in the seventies. Galton had no IQ scale with which to operate. On the other hand, 'objective' physical data was easily procurable and demonstrable.

When Galton began to consider heredity, when he began to consider the inheritance of normally distributed physical characters such as stature, he effected what is best thought of as a considerable revolution in statistical thought. Before outlining this, it must be said that in Britain in the sixties and seventies there existed no tradition of what we would call mathematical statistics. The nearest approach to be found was the 'error theory' tradition of astronomers, who were familiar with the mathematics of the normal curve, but in the context of a desire to use such mathematics to excise observational error from their work.³³ It is true that there did exist statistical societies: what was to become the Royal Statistical Society had been set up in 1834. But, these societies, in the sixties and seventies at least, were essentially information gathering bodies. As such they were doggedly anti-theoretical, and the London Statistical Society went so far in this 'Baconian' direction as to adopt for its emblem a sheaf of wheat decorated with a banner bearing the motto 'aliois extremum'.³⁴

Those who did apply the results of error theory to human data remained, nevertheless, in the grip of the metaphysics which surrounded error theory. Quetelet, for example, exemplified this to the point of seeing the mean man, 'l'homme moyen' as the ideal, error-free man. He made a virtue of what Galton would perceive as 'mediocrity'. Victor Hiltz³⁵ has shown that Quetelet was in the grip of a philosophy derived from the views of Laplace, which made both Laplace and Quetelet see deviations from the mean of a normal curve as due to 'accidental' causes, not subject to properly scientific study. But, for Galton the eugenicist, the normal curve was not a curve of error, but a curve of distribution. For Quetelet there could be no science of individual differences, but for Galton there could. This different

perspective was to lead Galton onto his greatest statistical discoveries - which dealt with the properties of deviations from the mean and with the way in which two or more distributions of these might be related.

Galton the eugenicist saw the normal curve with new eyes. If we take the distribution of 'natural ability', then people taking values in the left-tail of the curve would be seen as eugenically dangerous, and those in the right-tail as eugenically very sound. As Galton put it, 'these errors of deviations were the very things I wanted to preserve and know about'.³⁶ More generally though, he had fallen in love with the normal curve, which he called the 'supreme law of unreason', and which, he considered, the Greeks would have deified had they known of it. For,³⁷

whenever a large sample of chaotic elements are taken in hand and marshalled in order of their magnitude, an unsuspected and most beautiful form of regularity proves to have been latent all along.

Unsurprisingly therefore, Galton moved on to address the heredity of normally distributed human characters, taking height as the subject of a first study. This desire led him to spend a great deal of time and energy setting up anthropometric laboratories for the collection of human data, and eventuated in his finding that two generations of humans, in respect of stature, were related by linear regression.³⁸ That is to say, he found that if one took all the fathers in a population who deviated in respect of height by z inches from the population mean (in a positive or in a negative direction), then, if one took all of the sons whom they produced, one found that, on the average, these sons deviated by only az inches, in the same direction of deviation as the father. In his work, Galton fixed upon a value of a as equal to $1/3$, though later researches

resulted in a larger figure. He found moreover, that the variance of the groups or arrays of sons was the same for each array and was such as just to counteract the variance-contracting tendencies of linear regression between father and son. So, regression to the mean did not connote a progressive diminution of variance from generation to generation.

The general pattern of these discoveries can be garnered from adjacent tables and figures and their notes. Table 2 is of particular interest as it shows that the frequency distribution of pairs of sons and 'midparents' (Note, 'midparent' is a sort of parental average, and is explained in the note to Table 2) in a sample of observations carried out

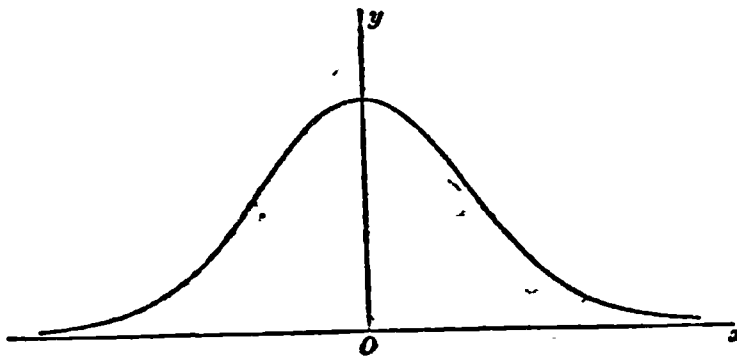


Fig. 1. A normal curve: the y axis denotes frequency, and the x axis the quantity -e.g., human stature - whose various values take the different frequencies indicated by the curve.

Image removed due to third party copyright

Table 1 A classification of men on the basis of the normal curve drawn up by Galton in Hereditary genius - the 'grades of natural ability' represent equal intervals along the x axis, measured in a 'standardised' metric - namely some multiple of the 'standard deviation'.

Image removed due to third party copyright

*Table 2 Taken from Natural inheritance. This shows the frequencies of
_pairs of midparent and child of different statures.*

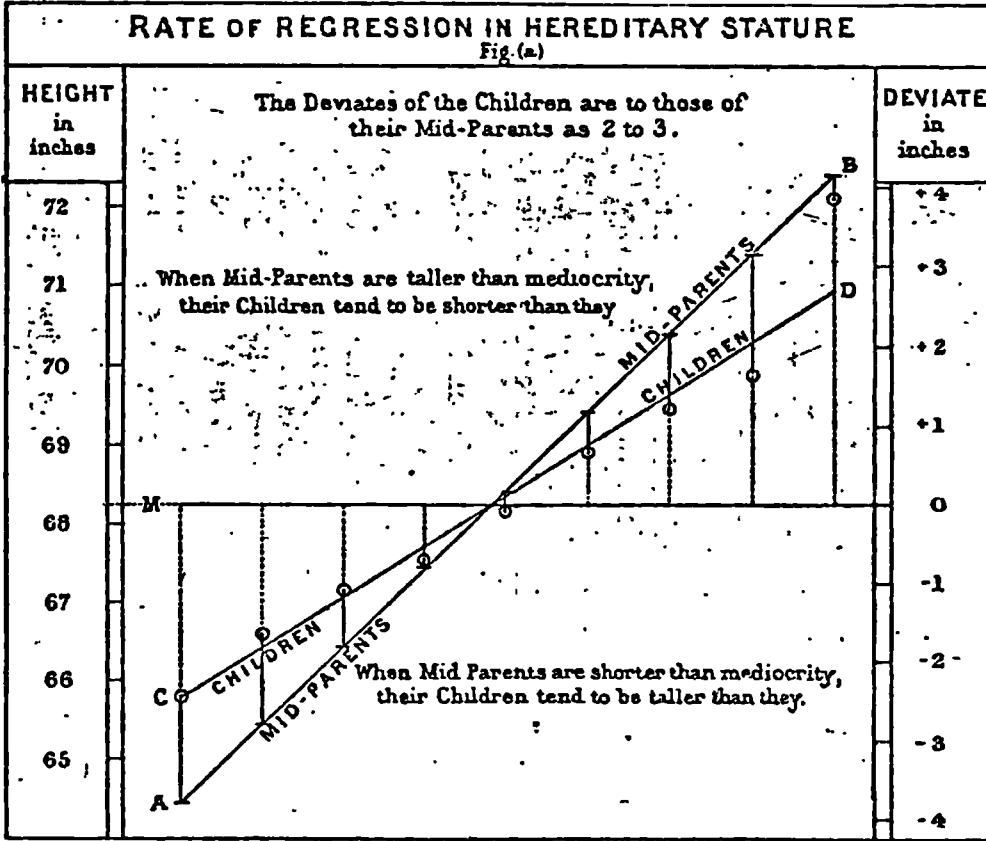


Fig. 2 This shows the salient facts of midparental regression. Mid-parents of deviation x have, on the average, offspring that deviate from the offspring mean by only $2/3 x$ units.

Figure 3 A bivariate normal surface of frequency. z is the axis of frequency, and x and y stand for the dimensions whose joint frequencies are given by the surface. In Galton's case, for example, they would stand for the statures of midparents and of sons.

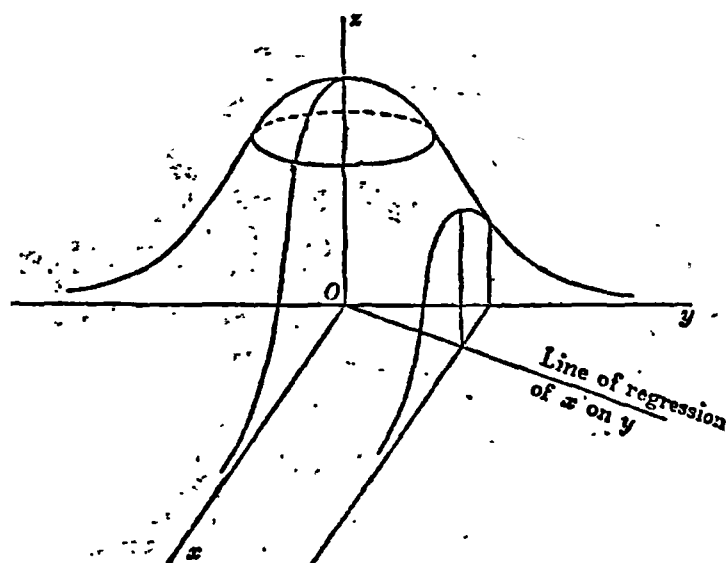


Image removed due to third party copyright

Fig. 4. Galton's frequency ellipses. This ellipse of constant frequency 'observed' by Galton corresponds to the ellipse of constant frequency indicated in Fig. 3.

on upper middle class Britons. One can see that, if drawn out as a surface of frequency, the data of Table 2 would give a surface corresponding quite well to the bivariate normal surface (Figs 1 and 3). Galton realised this approximation in the late 1880's, and caused some mathematical investigations to be made of the surface by a Cambridge mathematician.³⁹ Galton knew the equation for the surface because he realised that the joint distribution could be regarded as the product of the marginal distribution of mid-parents (normal, with known variance) and the conditional distribution of sons (normal, with known variance). For, just as the probability of a joint event - e.g., having two daughters, is the product of the probability of the one (eg. having a first daughter) times the conditional probability of the second (eg. having a second, given a first daughter), so the joint distribution of two variables (heights of mid-parents and sons) can be gotten by multiplying the relevant marginal and conditional distributions. (See figs. 2,3 and 4)

The mathematician in question was J. Hamilton Dickson of Peterhouse, Cambridge. Dickson, (1849-1931) had been fifth wrangler in the Cambridge mathematical tripos in 1874, and his answers to Galton's queries were published as an appendix to Galton's Natural inheritance of 1889. Let us briefly recall what it is that Dickson did. Galton told him the nature of his data. He had observed a marginal distribution of mid-parents with a normal form and a 'probable error' of 1.22 ins. He knew also that sons regressed linearly on their mid-parents with a regression coefficient of $2/3$ and were normally distributed about 'regressed means' with a probable error of 1.5. ins. In other words, the array of sons due to mid parentages deviating by d inches from the midparental mean would be normally distributed, with a probable error of 1.5 ins about a mean stature deviating by $(2/3)d$ inches from the general mean of sons. Thus Dickson could immediately write the equation for the surface of frequency for filial and mid-parental deviations jointly. For, multiplying these two distributions in accordance with the principle just described - that is to say, a normal distribution with probable error 1.22 ins and a conditional distribution, also normal, about a 'regressed' value of the midparental variable, he obtained the surface of frequency $z=f(x,y)$ which was such that 'the exponent, with its sign changed, of the exponential which appears in the value of z in the equation is, save to a factor

$$\frac{y^2}{(1.22)^2} + \frac{(3x - 2y)^2}{9(1.5)^2}$$

Dickson carried out a variety of investigations of such a surface. But one result, perhaps, was of the greatest importance. This was that the ratio of the squared probable errors of the two variables in the surface was equal to the ratio of the two regression coefficients. Thus, in the particular case of sons and midparents, we have the equality

$$\frac{(1.22)^2}{(1.7)^2} = \frac{\frac{1}{3}}{2/3} \quad (i)$$

This equality was to be very important for Galton, for it provided the key underpinning for his famous notion of mathematical correlation, given in 1888. This arose in the first instance out of Galton's acquaintance with a new system for the identification ^{of criminals} proposed by the French criminologist Alphonse Bertillon.⁴⁰ Bertillon's system involved taking a large number of anthropometric measures on criminals - eg, arm, leg, head measurements and so on. The underlying idea was that these measurements were independent ones. Galton was unhappy about this, for he felt sure that bodily measurements tended to ^{vary} together, although no one to his knowledge had attempted to measure the degree of correlation. It was a question that he collected data upon in 1888 in his anthropometric laboratory.

As soon as Galton began to tabulate results for the joint distribution of pairs of measurements taken on a large number of people, he found a very familiar pattern.⁴¹

No sooner had I begun to tabulate the data than I saw that they ran in just the same form as those that referred to family likeness in stature... A very little reflection made it clear that family likeness was nothing more than a particular case of the wide subject of correlation, and that the whole of the reasoning already bestowed upon the special case of family likeness was equally applicable to correlation in its most general aspect.

The problem of how to give a measure of the degree of correlation was solved in his 1888 paper on 'Co-relations and their measurement, chiefly from anthropometric data' in which he showed mathematically and empirically, that if the two variables in what we now call a binormal distribution were measured in terms of their own variance, then the regression of the first on the second would equal that of the second on the first.⁴² Furthermore, the 'regression' in such cases could not exceed unity. Thus, argued Galton, he had found a measure of the degree of correlation between two organs. It took the form of the coefficient of correlation 'r', which, as discovered by Galton, was a parameter in the bivariate normal surface. He was not much aware of the difference between population and sample values of the coefficient, was uncertain how to estimate its magnitude from any given set of bivariata data, and certainly did not invent the famous product-moment coefficient of correlation. We shall see the genesis of this in a later chapter.

Hence it was that Galton discovered the correlation coefficient, which opened up to social scientists the possibility of making mathematical connections between variables even though there was no strictly functional connection to be had. Galton realised, of course, that the array of x or of y (as the case might be) corresponding to a fixed value of y or x had the variances $(1-r^2)\sigma_x^2$ and $(1-r^2)\sigma_y^2$ respectively.⁴³

Two points perhaps should be made. The first is that Galton's discovery of correlation came only after he encountered the need for such a coefficient. In work on heredity he had been satisfied with using the regression coefficient, even though in cases where the two variables had different variances - eg., midparental and filial statures - the regression of the first variable on the second did not equal that of the second on the first. But, in forming a coefficient of correlation when considering the general problem of finding a mathematical measure of association, he was obliged to formulate a measure which made the association of x with y equal to that of y with x. The second point is that historians who have dealt with Galton's discovery of the correlation coefficient have depicted him as making the discovery empirically, by noting that when two variables were measured in terms of their own variances, the regression of the first on the second came to equal that of the second on the first. Doubtless this empirical aspect did exist, but as an explanation of Galton's discovery of the correlation coefficient its citation is on a par with explaining

Pythagoras' discovery of the right angle triangle theorem by referring to measurements which he may have made on fields. Just as we have to show how Pythagoras proved the universal truth of his theorem, so, in order to appreciate Galton's work on correlation, it is necessary to appreciate the use he made of Dickson's results, which clearly show the necessary existence of the correlation coefficient in every bivariate normal surface of frequency.⁴⁴

In subsequent chapters, we shall see that Galton's interpretation of the correlation coefficient differed somewhat from that given it by his various followers.

(f) The physiology of inheritance

Regression and correlation were discovered in the course of Galton's investigations into social biology. I have called his work 'social biology' because this seems to sum it up very neatly - he was investigating a biological topic, heredity, for social reasons. The biological discovery of linear regression was a very considerable one, as was the connected statistical discovery of the correlation coefficient. But, this aspect of his work did not exhaust Galton's contributions to biology. He was also interested in the theoretical side, in, so to speak, the physiology of heredity. Like Darwin, he wished for some account of the physiological mechanism by which hereditary characters were transferred from one generation to another. Like all scientists of the period (or, at least, like very many of them) he wanted to account for transmission and development in his theorising.

Darwin himself had addressed heredity in his study of 1868, with its theory of pangenesis.⁴⁵ Darwin's views on heredity were highly Lamarckian, in the sense that he allowed that there could be a significant inheritance of acquired characters, and held that changes in the environment were ultimately responsible for biological variation. His theory of pangenesis was an attempt to provide a physiological scenario for all of these processes. Reduced and simplified, the theory asserted that reproduction was the passing on of gemmules, small hereditary particles, germs capable of growing into adult features. The new individual was the product of the gemmules liberated by his parents, who, by turn, were subject to environmental influence. The individual was seen as liberating gemmules from all parts

of his or her body at all times, and these gemmules reflected the state of his body at the time of their release. Accordingly, when a character became developed by, say, the effects of use or disuse, a complement of gemmules reflecting this fixation in their propensities would be released, and could be passed on in the process of reproduction.

The beliefs that, somehow, heredity was a particulate process, that, on the whole, it yielded forms intermediate between the parents and that the process was subject to environmental interference were generally held during much of the second half of the 19th century. Mendel's ideas, of course, went against this trend, but, his ideas went forgotten until the turn of the century in 1900. The general point can be made by referring to Spencer, whose quasi-environmentalism has already been mentioned.

Now, if some form of quasi-environmentalism, some belief in the inheritability of acquired characters was quite general in Spencer's and Darwin's days, there was a shift away from this position at end-century due to the ^{introduction of ideas of me} continuity of the germ plasma in the 80's. Weismann's view, however, was backed not by 'direct' empirical evidence - some microscopical demonstration for example - but by plausibility arguments of one sort and another, whose forcefulness depended upon many things, including an acceptance of his views about the material bases of heredity in the chromosomes of the cell.⁴⁶ The rate at which 'Weismannism' overthrew 'Lamarckism' in the late 19th and 20th centuries remains unchronicled, though it is clear that Weismann had made great strides by 1910. But, right up until 1910 it was quite easy to find reputable Lamarckians, especially perhaps in America, where neo-Lamarckianism had been a significant development in evolutionary theory.⁴⁷

Weismann and Spencer debated the issue in the 1890's in the Contemporary review, with Spencer for one admitting that⁴⁸

a right answer to the question whether acquired characters are or are not inherited underlies right beliefs not only in biology and psychology, but also in education, ethics and politics.

We may return to this point at a later juncture. For the moment it should be noted that Galton's first move into the field of the physiology of heredity led to a brief controversy with Darwin. Galton, no doubt keen to discredit the idea that acquired characters might be inherited, sought to show that gemmules did not travel in the blood stream. This he did by

transfusing the blood of one ~~breed~~ of rabbit into that of another immediately before the mating of the rabbit which had received the transfusion. Galton reasoned that if gemmules were carried in the bloodstream, the rabbit receiving the blood would receive a complement of gemmules also, and, since the rabbits were chosen for dissimilarity, would show the effects of this in the nature of offspring produced. No startling progeny were in fact conceived, and Galton concluded against gemmules in the blood. Darwin responded by denying that he had ever claimed that the blood was the site of gemmule transfer within the body. Galton withdrew his criticisms of pangenesis.⁴⁹

This episode, however, did not prevent Galton from going on to offer his own theory of the physiology of heredity, which resembled Darwin's in many points except that it allowed very little scope for the inheritance of acquired characters. Roughly speaking, Galton suggested that in each zygote there were several germs contending for development into each character and feature of the adult, with each germ being derived from one ancestor.⁵⁰ Those germs that did not develop or become 'patent' went to form the generative organs of the individual formed from those that did. Thereby, these germs could be passed on for another chance of 'patency' or, failing that, for another round of transmission in 'latent' form. In this way, the germ-plasm was seen as being separate and unto itself and relatively immune from the activities and influences attending the body in which the germ plasm was contained. The system proposed by Galton was not dissimilar to Darwin's, but was reorganised in a manner which made the inheritance of acquired characters an improbable event. Wallace recalls in his autobiography that he was a quick convert to the idea of the non-inheritability of acquired characters, despite his original regard for Darwin's pangenesis, and that Galton's work had a great effect on him, materially affecting his view of Darwin's system.⁵¹

It is only with some knowledge of Galton's physiological system of heredity that we can understand what was perhaps his most famous contribution to the science of heredity in the 19th, century, namely the so-called 'law of ancestral heredity'. This appeared in his work in the early 1880's, and was generally presented as a derivation from statistical data - for the inheritance of continuously varying characters such as height. The derivation however, is a mere tissue, and is certainly invalid.⁵² The law

was really a restatement of an old halving thesis, being based on the idea that there must be a transmission of only a half of the germ plasm of each parent at reproduction - otherwise, successive generations would contain successively doubled quantities of germ plasm.

As stated, the law claimed that⁵³

the influence, pure and simple, of the mid-parent may be taken as $\frac{1}{2}$ and that of the midgrandparent as $\frac{1}{4}$ and so on. Consequently, the influence of the individual parent would be $\frac{1}{4}$ and of the individual grandparent $\frac{1}{16}$ and so on.

Now, as noted, this rather vague claim was presented as a derivation from values for the regression of son on parent in respect of continuously varying characters. The derivation was invalid, but, interestingly, Galton's claim was applied by him to discontinuous attributes - such as eye-colour - where he supposed that $\frac{1}{4}$ of the members of a family would exactly follow each parent, that $\frac{1}{16}$ would follow each grandparent, and so on. The connecting link, enabling Galton to feel confident in moving from continuous to discontinuous characters was, I take it, his physiological theory of heredity - for certain passages in his writings on the physiology of heredity suggest very strongly that he thought of the 'influences' in the quotation above as being complements of hereditary particles.⁵⁴ Thus, in his mind, his principle had a twofold application - to attributes and to continuously varying characters. Also based in this theory, and in several rich analogies and metaphors, was a belief that crucial evolutionary changes might be large and discontinuous - being the result of quantum-like ~~moves~~ of the particulate arrangement from one 'position of stability' to another.

(g) Psychology

So far I have mentioned Galton's statistical and biological thought, showing, I hope, how they related to one another and to his broad overall eugenic and social Darwinian concerns. Now, to complete a discussion of his 'scientific' work it remains only to allude to his labours in psychology which are remarkable on three counts. First, there is his idea of 'natural ability' and its relation to simple sensory processes. Second, there is his work on elementary mass mental testing, and, finally, there is his work on twins and related topics, undertaken to drive home the message of nature rather than of nurture.

Above all, Galton was interested in individual differences in mental ability, and, of course, in the heritability of the same. Thereby he differed from continental experimentalists, who sought the universal not the varying features of psychological life. There is no reason to suppose that, in psychology, he was a deep or systematic thinker, but he must be appreciated as the man who put on the map the idea of a psychology of individual differences, associated with the idea that the differences were due to heredity rather than to environment. In this department he is perhaps most notable for his insistence that men can be classed according to a single metric - of what he termed 'natural ability' - first promoted in Hereditary genius in 1869. This, he assumed, would be found to follow a normal curve in all races, and the mean level attained by any race would be reflected in the proportion of great men that it produced. Thus, the negroes, having produced few men adjudged by Galton as of great mental gifts, could be seen as having a lower mean than the British, who, by turn, were unfavourably compared with the Athenians, who produced a high proportion of great minds, among a populus whose tastes, Galton noted, were much higher and ^{more} refined than those of the British populus - a fact ascertainable by referring to the contents of any railway station news-stand.⁵⁵ The argumentation surrounding this concept was, as frequently with Galton, very bad, but the concept ^{was} powerful if vague - the idea of each member of a community having a certain numerically assessable 'natural ability', which would serve as a measure of his eugenic worth. This, we shall see, was a theme that was to recur time and again in the Galtonian tradition. Later, when we come to discuss Galton's work in the field of the popularisation of eugenics, we shall see the way in which the supposedly normal curve of 'natural ability' was related to the different social grades and classes which Galton discerned in the British population.

Now, exactly what Galton meant by 'natural ability' is hard to say. It was, in one account, comprised by zeal, capacity and the power of work.⁵⁶

- By natural ability, I mean those qualities of intellect and disposition, which urge and qualify a man to perform acts that lead to reputation. I do not mean capacity without zeal, nor zeal without capacity, nor even a combination of both of them, without an adequate power of doing a great deal of very laborious work. But I mean a nature which, when left to itself, will, urged by an inherent stimulus, climb the path that leads to eminence, and has strength to reach the summit - one which, if

hindered or thwarted, will fret and strive until the hindrance is overcome and it is again free to follow its labour-loving instinct. It is almost a contradiction in terms, to doubt that such men will generally become eminent. On the other hand, there is plenty of evidence in this volume to show that few have won high reputations without possessing these peculiar gifts. It follows that the men who achieve eminence, and those who are naturally capable, are, to a large extent, identical.

This is not much to go on, but fortunately, at other places, Galton did specify the natures of some of these ingredients of 'natural ability' in a little more detail. His thinking had two tracks - sensory discriminatory power and 'energy'. Let us take them in order.

His thoughts on sensory discriminatory power were laid out in his Inquiries into human faculty of 1883. Here, in a section on 'sensitivity', he argued that the senses were the only sources of information, and consequently, that⁵⁷

the more perceptive the senses are of difference, the larger is the field upon which our judgement and intelligence can act.

Idiots, he wrote, had a low discriminatory power, whereas a former Lord Chancellor had the most amazing power. This seemed to point, along with other evidence, to the superior sensory power of the intellectually gifted. This hypothesis was further confirmed by observations suggesting that African natives did not have the exceptional powers of observation sometimes attributed to them by travellers, and by the fact that merchants infrequently employed women as tasters. Galton, it seems, was not an advocate of women's emancipation. His overall conclusion was that,⁵⁸

a delicate power of sense discrimination is an attribute of a high race, and that it has not the drawback of being necessarily associated with nervous irritability.

Galton's thoughts on energy were presented in the same work as were his thoughts on sensory discrimination. Once again, they are imprecise, but suggestive. Energy, he said, was 'the capacity for labour'. It was an attribute of 'higher races' he said, and he had found that the leaders of scientific thought in Britain were 'generally gifted with remarkable energy, and that they had inherited the gift of it from their parents and grandparents'. Once again he concluded,⁵⁹

In any scheme of eugenics, energy is the most important quality to favour; it is as we have seen, the basis of living action, and it is eminently transmissible by descent.

Finally therefore, and putting together the diverse elements, it would seem that Galton's idea of natural ability was somehow compounded out of fine sensory powers and a very high level of energy. Perhaps he had in mind a model of natural ability as due to (a) a powerful ability to take in information, and (b) an energetic and strong processing faculty. But, sometimes it is better to see what followers make of ideas than to pursue the intentions of their leaders, and this strategy will be followed here in a later chapter.

Knowledge of Galton's concern for sensory powers and his estimate of the connection of these powers with eugenic work helps us, I think, to understand something of the rationale for some of the developments which he made in mass mental testing. For in a series of anthropometric laboratories, the first of which was established at the International Health Exhibition of 1884, Galton tested some of the physical and mental properties of many thousands of people. The tests included keenness of sight and of hearing; colour sense; judgement of eye; breathing power; reaction time; strength of will and of squeeze; force of blow; span of arms; height, both standing and sitting; and weight. Significantly, his account of this first laboratory was published in the Journal of the Anthropological Institute for 1885,⁶⁰ and we know that he took advice and assistance from Croom-Robertson, Grote professor in the University of London, and editor of Mind. Galton measured some fairly simple functions in his anthropometric laboratories, and the final value of his measurements is not at all clear, but, he did attract the discipleship of James McKeen Cattell,⁶¹ the American psychologist, who worked with him for some time, after a period of work with Wundt, wrote a paper on their joint anthropometric work for Mind, and pioneered psychometric work in the United States, failing, however, to find the hierarchy of test scores which Charles Spearman was later to insist upon as demonstrative of the existence of a true central factor 'g' in intelligence.

The third strand of Galton's psychological work lay in the pioneering of twin studies and allied investigations. Once again the hope behind the enterprise was to show the priority of nature over nurture in matters of mental constitution and ability. One such exercise was written up in his work of 1874, English men of science, their nature and nurture,⁶² which presented the results of a questionnaire-based inquiry into the ancestry, personal qualities, intellectual biographies and education of leading English men of science. This was undertaken partly in response to de Candolle's Historie des sciences at des savants depuis deux siècles, which stressed,⁶³ contra Galton, the social rather than the biological sources of scientific excellence. De Candolle put his money on social factors such as 'freedom to state and publish all opinions, at least on scientific topics, without experiencing serious harm'. Galton we know, put his faith ~~in~~ inherited energy. From his work, Galton felt able to stress again the importance of heredity, and, in English men gave the first strong expression of his hope that the 'gigantic monopoly' of the established church might be eliminated, apparently in Galton's hopes to be replaced by another monopoly - of science.

This was to be⁶⁴

a sort of scientific priesthood throughout the kingdom, whose high duties would have reference to the health and well-being of the nation in its broadest sense, and whose emoluments and social position would be made commensurate with the importance and variety of their function.

But, though this form of psychobiographical investigation has thrived fairly well, notably at the hands of Lewis Terman the famous American exponent of mental testing and constructor of the ~~Stanford~~^{Binet} scale of IQ, it has not had the influence of another Galtonian tradition - that of twin studies, now so much in the news after the realisation that one of Galton's leading followers in this area - the Galtonian psychologist Sir Cyril Burt, 'adjusted' his data on occasions.

Galton's own twin studies were discussed in his Inquiries into human faculty. He was acquainted with the difference between twins of what we would call monozygous and dizygous origins - referring to the former as 'due to the development of two germinal spots in the same ovum'.⁶⁵ His method of inquiry was strictly non-quantitative, and consisted in obtaining information about the development of pairs of twins brought up in different ways. The details found are fascinating to read, and the conclusion is the standard Galtonian one.⁶⁶

There is no escape from the conclusion that nature prevails enormously over nurture when the differences of nurture do not exceed what is commonly to be found among persons of the same rank of society and in the same country. My fear is that my evidence may seem to prove too much, and be discredited on that account, as it appears contrary to all experience that nurture should go for so little.

Here interestingly, it is almost as if Galton were rehearsing a subsequent controversy - namely that of whether high in-group heritability tells one anything about the heritability of between-group differences. And, though his observation on the role of social groupings might seem to put him into the position of thinking that inter-group differences might be due more to environment than to heredity, he never recanted, as far as I know, on his early explanations of inter-racial differences in genetic terms.

(h) Institutions

Finally, then, we have seen what might be termed the 'scientific' side of Galton's work - though, of course, the more we look at it, the more it becomes clear that his scientific work and his politics were closely joined. He adopted a cosmology, a picture of the world, complete with inbuilt moral directives at an early stage, and spent the remainder of his scientific career attempting to articulate the picture - studying human heredity and the human psyche, looking always for an opportunity to show the superiority of nature over nurture. Galton was not one to unduly expose his theories to harsh criticism or analysis, and it is perhaps significant that he soft-pedalled the doctrine of eugenics for thirty years after its first appearance and poor reception in the 1865 paper. He had to wait for the tide of opinion to turn before he would take to the platform and fight for his cause in open debate. However, in the 'scientific' articulation of his world-view, he made several vitally important innovations in statistics, psychology, the study of heredity and in sociology. In statistics we have the idea of the correlation coefficient, which Pearson described as lying at the root of the subsequent development of mathematical statistics. In psychology he pioneered twin studies and mass mental testing. In the study of heredity he threw doubt upon the inheritance of acquired characters and discovered linear regression between parents and offspring, and, indeed, between pairs of 'collateral' relatives - eg nephew and uncle, and so on. In sociology he pioneered the questionnaire. It was, intellectual

speaking, a most important career, making up in width and range what it lacked in careful analytic power and ability to criticise the assumptions which underlay his work. It is interesting, for example, that Galton offered no reply to Huxley's essay on evolution and ethics, though one might think that Huxley's oration undermined the basis of much of Galton's thought.

But, what of Galton as an institutional figure? I have mentioned that he was a fellow of the Royal Society, that he was a large figure in the Geographical Society, in the Anthropological Institute of Great Britain and Ireland, and in the British Association, and that he was the friend of men like Spencer and Tyndall, who, in their various ways were allied with him in his desire to replace the clergy with a scientific clerisy. These were significant figures in Victorian culture. It is not by accident that W.H. Mallock's satire, the New republic, contains characters corresponding to Tyndall, Huxley and Clifford.⁶⁷ In this closing section of the chapter it remains to consider Galton's institutional labours in science, particularly as they related to his concern for eugenics. Most of these activities were crammed into the last decade of his life, that is to say, into the Edwardian era, and these we shall shortly see. But they do not exhaust his institutional work, successful or attempted.

Like most scientists, Galton was anxious to stimulate the creation of a set of workers labouring in his own chosen vineyard - in this case the statistical investigation of eugenics, heredity and psychology.

But, there being at the time no SRC to whom he could apply for the establishment of a research unit, and there being very little in the way of institutionalised science in Britain - at least until the end of the century - the prospective numbers of labourers was limited. Galton himself, in any case, always preferred free associations of independent workers to distinctive schools. But, whenever possible, he would follow up chances of collaboration with others. One such was the economist, F.Y. Edgeworth, with whom Galton had correspondence in the late 80's.⁶⁸ But, as Donald MacKenzie has noted, there was little meeting of minds between the two men.⁶⁹ Edgeworth, the mathematical economist, author of a hedonistic calculus, and of Mathematical psychics was simply not tuned in to Galton's wavelength. As an economist, he could not but consider that the problems facing society were resolvable by economic reform rather than by eugenics - whereas Galton, more of a visionary perhaps, stressed the need to reform people rather than the system of relations within which they lived out their lives. Edgeworth may have had some interest in Galton as a patron, but it is interesting to note that when he reviewed Galton's

Natural inheritance of 1889 for Nature, he praised the mathematical methods rather than the eugenic 'hidden curriculum' of Galton's work,⁷⁰ and that, though he did a certain amount of work on the calculus of correlations, he never became a wholehearted follower of Galton. Shortly we shall see something of the way in which, and of the time at which, Galton could begin to collect true followers.

A parallel episode concerns Florence Nightingale, whose formidable administrative powers were buttressed by a belief in the efficiency and administrative utility of statistical knowledge. She was a fervent student of the works of Quetelet, and her copy of the Physique sociale is annotated on almost every page. Moreover, the frontⁱpiece of her copy bears the following inscription in her own hand (it was a presentation from Quetelet).⁷¹

The sense of infinite power
the assurance of solid certainty
the endless visits of improvement

Nightingale knew of Galton through his works on camping and travelling and his concern to teach the British army how to live in rough country. But her main contact with Galton came in the early 1890's. Nightingale hoped to have established at Oxford a chair of 'social physics' (i.e., statistics) at Oxford. Her aim was to establish some form of social assay. She wanted to assess the benefits of Forster's education act, the results of punishments in jails, the efficacy of poor-law administration and of its current relations with charities, the progress or otherwise of the Indian nation, and so on. Nightingale wanted Galton to assist in drawing up plans of research for the professorship, but Galton was very unsympathetic. He considered that a professor, being tenured, would be likely to^{do} little real work, and that such inquiries were best performed by committees of independent men. If Nightingale really wanted a professor, he said, it would be better to appoint one at the Royal Institution. Galton it seems, was, at heart a pre-professional. Pearson asks of his attitude 'how could a school of trained applied statisticians have been created by six lectures a year at the Royal Institution'? But, one suspects, the notion of such a school was somewhat repellent to Galton's view of science, which appears to have been that of the independent man choosing of his own volition to read the book of nature in the manner he preferred. Enthusiasm was one thing, 'training' was another. The Nightingale scheme met with no enthusiasm for him, and was finally extinguished.⁷²

Galton's real achievements in the field of institutions lay in his connections with eugenics. His pathway into this sort of activity was two-fold, roughly speaking academic and popular, and his twofold path resulted in the creation of mutually hostile institutions.

If we focus at first on the academic path, we find that, in the late 1880's, Galton began to enter correspondence with W.F.R. Weldon, professor of zoology at University College London. We shall see more of this relationship in successive chapters, where some of their correspondence is reproduced. But, for the present, the point which should be noted is that Weldon was a biologist anxious to promote a new style of evolutionary biology. He wanted to get clear away from the morphological paradigm which will be discussed more fully later on. He hoped, in short, to revolutionise the study of biology, particularly of evolution, by the application of Galton's statistical methods to the study of wild populations of animals. He started in quite a humble way, showing that distributions among shrimps in the wild followed the familiar normal curve. This was a novel observation at the time, and clearly opened up an imitative research programme. There was the law of ancestral heredity to apply, and the correlation coefficient to use in these studies of wild populations. Weldon did not stint, and came into closer contact with Galton as he had discussion after discussion with Galton about the statistical side of his work. Weldon was far from being mathematically illiterate, but, being a biologist of the period, was not expert in mathematics. Thus, with Galton's assistance, Weldon began to found a science which soon grew into a discipline known as biometry.

The growth occurred in good part because Weldon met up with a colleague at University College London, Karl Pearson, then (1890), professor of applied mathematics and mechanics. They appear to have come into close contact through their joint interest in University reform, and, before long, Pearson was assisting Weldon with his biometry. Soon, a little assistance grew into the work of an equal or possibly leading partner, and Pearson began to publish, in the Philosophical transactions of the Royal Society for the most part, a series of works on the 'mathematical theory of evolution', in which he explored various aspects of heredity and variation, and the statistical problems which, in his view, they raised. The details of all of this will be dealt with in the next chapter; suffice it to say for the present that Pearson

slowly built up a 'biometric school' of students in his applied mathematics department who took his courses in statistical biology or 'biometry'. His lectures, whose audience included men such as G.U. Yule the great author of a standard statistical text-book, were the first lectures in mathematical statistics given in Britain, or indeed, in a strong sense, anywhere.

Pearson too grew increasingly friendly with Galton, sometimes establishing contact with the old man by highlighting their common possession of some quaker ancestors, and, as the nineteenth century passed into the twentieth, became more and more the man who was ^{seen to be} continuing with central areas of Galton's research programme. Most importantly, perhaps, Pearson showed himself to be sympathetic to Galton's views on eugenics - though there are certain key differences in their orientations, as might be expected when it is recalled that both were men of their periods, and that between their birth dates there is a gap of thirty-five years. In later chapters I will trace their growing relationship and mutual interactions. The turning point for both men seems to have come in 1901, when we find Pearson writing to Galton that ⁷³

It would be a very great pleasure to me to know you were going to take the field with regard to what I am convinced is of the greatest national importance - the breeding from the fitter stocks. If one could only get some one to awaken the nation with regard to its future! The statesmen, who really have the ear of the populace, never think of the future. They will not touch the issue of coal supply nor that of fertility, and yet I am convinced these are far more important for the very existence of the nation than any question of government, church discipline or even technical education!

And, from that time onwards, we find Galton, then almost an octogenarian, taking to the public platform to publicise the cause of eugenics. Pearson's biography of Galton contains a long section on 'Eugenics as a creed and the last decade of Galton's life'. Pearson notes that 'it was not till the beginning of the present century that he (Galton) considered the time ripe for a more general public appeal, or sought proselytes to the new faith'. In this, Pearson is correct, and, in later chapters I will consider what it was about this period (the turn of the century) which could have emboldened Galton to feel that the time was at last ripe for the beliefs he had been nurturing for the last thirty-five years.

on the 'intellectual' side, Galton became very busy in the eugenic sphere during this decade after 1900. 1901 saw Galton giving the Huxley lecture and receiving the Huxley medal of the Royal Anthropological Institute. This ^{lecture} was remarkable for his essay into the issue of the relation between class and natural ability, now retitled 'civic worth'. In his lecture Galton was happy to make correlations between class and social or civic worth - regarded as an inherited commodity - and made great use of the sociologist Booth's recent analysis of the population of London into different social groups.⁷⁴ People in the left-tail of the now familiar normal distribution he identified as corresponding to Booth's classes A and B, that is to say, 'criminals, semi-criminals and loafers, and the casually employed poor, often suffering from shiftlessness and excessive devotion to drink'. The right tail of the curve of civic worth he identified with independent professionals and large employers. The different values of men from the two classes were assessed in terms of the market price of babies of different sorts - potential upper professional men, for example, being worth to the nation many thousands of pounds as compared to the worth of five pounds for a labourer's baby as computed by the statistician Farr.⁷⁵ And, making his central point, Galton stressed the need to encourage breeding among the gifted, a priority which he put over that of stopping the reproduction of the lowest orders. He favoured what was to become known as positive eugenics ^{as} negative eugenics, and had in mind schemes to promote early marriage amongst the most talented, suggesting that the fourteen million pounds in charity then spent annually might be diverted to support a venture in positive eugenics. In 1904, he was able to address the newly founded Sociological Society on the topic of 'Eugenics: its definition, scope and aims'. This attracted lively comments from figures including H.G. Wells, Benjamin Kidd, William Bateson, Bernard Shaw and C.S. Loch.⁷⁶ Shaw, interestingly, was most enthusiastic, and wrote that there was now 'no reasonable excuse for refusing to face the fact that nothing but a eugenic religion can save our civilisation from the fate that has overtaken all previous civilisations'. His response was not entirely

unrepresentative. In the following year, there was another paper to the Society, on 'Restrictions in marriage', with comments from men of the stripe of Westermarck and A.C. Haddon, organiser of the famous Torres Straights expedition. Once again, in the strongest language, there a call for the adoption of eugenics as a new religion.⁷⁷ Eugenics, Galton said,

sternly forbids all forms of sentimental charity that are harmful to the race, while it eagerly seeks opportunity for acts of personal kindness, as some equivalent for the loss of what it forbids.

In 1907 he was invited to Cambridge to give the Herbert Spencer lecture, again delivered on behalf of eugenics.⁷⁸ Thereafter Galton was able to give only a few more papers on eugenics due to his age and infirmity, and he died in 1911.

But, his last decade was marked not only by 'intellectual' moves but also by concrete institutional ones too. Galton, it should be stressed, was an extremely wealthy man. He had produced no children himself, despite his eugenic commitments, and was in a position to spend. The first signs of his preparedness came in 1904,⁷⁹ when Galton wrote to Sir Arthur Rucker, principal of the University offering 500 pounds a year to establish the 'exact study of what may be called National Eugenics', by which Galton meant 'the influences which are socially controllable, on which the status of the nation depends'. The University responded by the setting up of a committee consisting of Sir Edward Busk, Galton, Pearson and Halford Mackinder. The outcome was a decision to appoint a Galton research fellow and a Galton scholar. The fellow's duties were as follows

"(a) To acquaint himself with statistical methods of inquiry, and with the principal researches that have been made in Eugenics, and to plan and carry out further investigations.

"(b) To institute and carry on such investigations in the history of classes and families as may be calculated to promote the knowledge of Eugenics.

"(c) To prepare and present to the Committee, though not necessarily for publication, an annual Report on his work (to be done under general direction of the Committee). To give from time to time, if required or approved by the Committee, short Courses of Lectures on Eugenics and in particular on his own investigations thereon.

"(d) To prepare for publication at such times and in such manner as may be approved by the Committee (and at least at the end of his tenure of the Fellowship), a Memoir or Memoirs on the investigations which he has carried out".

The first fellow was Edgar Schuster, nephew of the famous physicist Schuster. He was a pupil of Weldon, who, in 1900, had gone to take the chair of zoology in Oxford, ^{Schuster} stayed for 3 years, being replaced⁸⁰ in 1906 by David Heron, who was for long to be Pearson's right-hand man. In 1906, Galton confided in Pearson that he, Galton, was going to leave⁸¹ a very large sum of money to the University to endow the 'furtherance of the study of national eugenics', which he now defined as 'the agencies under social control that may improve the racial qualities of future generations either physically or mentally'. The establishment from which Schuster worked was known as the 'Eugenics record Office', and it soon had an American imitator,⁸² In 1906, this record office passed out of Galton's direct supervision and into Pearson's control, changing its name, en route, to the 'Galton Laboratory for National Eugenics'.⁸³ Galton gave it a further dowry of £1,000. Its staff now included Heron, Ethel Elderton, sister of W. Palin Elderton the actuary and Amy Barrington. So, Pearson, still professor of applied mathematics and mechanics, now controlled, within his department a biometric laboratory (of which, more below) funded by the Drapers' Company,⁸⁴ which was a centre for statistical and biometrical research, and an Eugenics Laboratory, financed by Galton. This was not all, for in 1900, with Weldon, and with Galton's financial assistance once again, he had founded Biometrika, a journal for the 'statistical study of biological problems'. Weldon died in 1906, leaving Pearson in charge as sole chief editor. Soon Pearson began also to publish a series of biometric and eugenic memoirs. He put out a 'memoir series', commencing with Schuster's Inheritance of ability of 1907, a 'lecture series' beginning with his own Scope and importance to the state of national eugenics of 1909, a series on 'Questions of the day and fray', commencing with Pearson's Influence of parental alcoholism on the physique and ability of offspring, and also a series of 'Studies in national deterioration', commencing in 1906 with David Heron's study of the Relation of fertility in man to social status and on the changes in this relation that have taken place in the last 50 years.

Pearson then, by 1906, had in a very strong sense taken up Galton's research programme. Only one more step in this direction remained, and it was taken on Galton's death in 1911, when it was found that Galton had left the residue of his estate to endow a chair of eugenics in the University of London, with first refusal to be given to Karl Pearson. He accepted, and,

taking the biometric and eugenics laboratories with him, left the chair of applied mathematics to form a new department of applied statistics in which he sat as the Galton professor of eugenics. In his person then, by 1911, statistics, eugenics and biometry were all officially represented. He held the chair till 1933, when it was taken by R.A. Fisher.

But, in a sense, Pearson was not the sole heir. For there had also been formed, in 1907, a Eugenics Education Society.⁸⁵ This was an offshoot of another organisation, the Moral Education League. The Society, as we shall see in a later chapter, rapidly increased in membership and influence in the years prior to the first war, and was in fact able to stage a huge international conference in 1912. The significance of this popular movement is that it took its lead from Galton, and was able to obtain his services as honorary president. We shall see that the Eugenics Education Society was notably more populist than Pearson's more 'academic' department, and that there were frequent clashes between the two groups, both marching, though in slightly different directions, under the flag of Galton.

So, at his death, Galton had made significant contributions to statistics, heredity and psychology, and had set up or inspired a series of eugenically minded institutions which oversaw the development of these subjects in the early years of this century. The remainder of this work traces these developments.

Finally then, what are we to make of Galton? We can see, I think, that eugenics was a matter of overwhelming importance for him. It motivated his work in heredity, and, by making him see the extreme or eugenic man as something other than an error, and as a fit subject for scientific investigation, ^{led him} ^{in moral statistical matters.} Thereby he was led to the discovery of regression and correlation, both, so to speak, as statistical and as biological phenomena. Eugenics, based by turn on a high regard for nature over nurture, and tied in with beliefs about the bases of intelligence, led him to pioneer various forms of psychology, including mental testing, and, quite generally, to uncork the bottle of differential psychology which at the time of writing, is undergoing an interesting appraisal in the correspondence columns of the Times.⁸⁶

What then of Galton's conversion to eugenics? We see that it was a component of a massive mental shift in which he transferred his allegiances from a Christian cosmology to a rather ill thought out 'Darwinian' perspective on everything, including morals. Why Galton should have become so keenly attached to the principle of mental heredity isn't easy to understand. Certainly, the evidence which he managed to accumulate for the

proposition was never compelling, except to the converted.

He was, we know, an anti-egalitarian conservative with philanthropic desires, a man who combined a number of kindly instincts with a propensity to begin his scientific works with passages such as the following:⁸⁷.

I have no patience with the hypothesis occasionally expressed, and often implied, especially in tales written to teach children to be good, that babies are born pretty much alike, and that the sole agencies in creating differences between boy and boy, and man and man, are steady application and moral effort. It is in the most unqualified manner that I object to pretensions of natural equality.

Galton's eugenic ideas, predicated upon other ideas about human worth and progress, fitted with his conservative outlook, with his tendency to dismiss 'primitive' races, and so on. We cannot explain any of these views as the 'natural' or 'inevitable' consequences of his social position - which, in any case, is complicated by dual roles as rentier on the one hand and as defender of the professional on the other. To take just one point, ^{the} the now familiar one of Galton's insistence upon the non-inheritability of acquired mental characters, we can see that others of similar background - notably Darwin - found no problem in taking a different line, and, more significantly, as we shall see in the chapter on eugenics, others of conservative disposition and reformist biosocial ideas were able to combine these stances with some commitment to the heritability of acquired characters.

At the same time, we can see that Galton's theoretical perspectives did not sit ill with his social allegiances. His views, in short, were one out of a number of possible set of ideas, each of which might have harmonized with the interests indicated by his social position. Why he selected the set that he did select is a matter, one presumes, best explained by reference to the particularities of his personal development. At the moment however, the state of the art or science of psychobiography is not such as to give one confidence that the particular episodes and events that one might cite - notably perhaps, ^{the stress caused by} Galton's father's continued emphasis on the need for academic prowess - are ones that ^{constitute} a genuine explanation of his later selection of a perspective.

NOTES

Chapter 2

1. See Speeches at a dinner held in University College London, in honour of Professor Karl Pearson, privately printed, Cambridge (1934). For Pearson on Galton generally see Karl Pearson, The life, letters and labours of Francis Galton, ^{3 vols} Cambridge, (1914-1930).
2. For an account of the progress of phrenology, see R.M. Young, Mind, brain and adaptation in the nineteenth century, Oxford, (1970).
3. Galton archive, University College London. See item 81.
4. For a brief account of Galton's life see D.W. Forrest, Francis Galton, The life and works of a Victorian genius, London, (1974).
See also R.E. Schofield, The lunar society of Birmingham: A social history of provincial science and industry in eighteenth century Europe, Oxford (1963).
5. The best source is F. Galton, Memories of my life, London (1909).
6. See Pearson, op.cit. (note 1), Vol. I, 177.
7. Ibid., Vol. I, 155.
8. Ibid., 209.
9. F. Galton, Narrative of an explorer in tropical South Africa, London (1853).
10. B. Webb, My apprenticeship, Penguin edu. (1971), 150.
11. Darwin himself was most impressed by the fruits of natural theology, see e.g. Darwin's autobiography, ed. G. de Beer, London (1974), 32.
12. Cited in Pearson, op.cit. (note 1), Vol. I. plate 2.
13. These and other details taken from Arthur Keith, 'Galton's place among anthropologists', Eugenics review, 60, (1968), 12-24. See also G.W. Stocking jnr., 'What's in a name? The origins of the Royal Anthropological Institute (1857-71)' Man, n.s.6 (1971), 369-390.
14. Ruth Cowan notes in her Ph.D. thesis on Galton that Galton frequently presented himself as a philanthropist, as a man who wished to be 'of use in the world'. Indeed, as Cowan points out, the first paragraph of Hereditary genius speaks of the 'duty we owe to humanity', and, in his Memories pp.321-322, Galton speaks of the nature of the 'true philanthropist'.
15. See Galton, op.cit (note 9).
16. Cited in J.S. Haller jnr. Outcasts from evolution, Chicago (1971), 4.

17. Anthropological review, 4, (1866), 120.
18. F. Galton, 'Hereditary talent and character', Macmillan's magazine, 12 (1865), 157-177 and 318-327.
19. Ibid, 165-166.
20. Galton did not explain how the first members of ^asociety to show these altruistic traits would prosper. A modern perspective on similar, but not identical, issues is to be found in R. Dawkins, The selfish gene, Oxford (1976).
21. Op.cit, (note 18), 322.
22. See F. Galton, 'The part of religion in human evolution', National review, August 1894, 755-765.
23. For a discussion of Tyndall's views see D.C. Somervell, English thought in the nineteenth century, London (1929), 134-135.
See also F.M. Turner, Between science and religion, London (1974).
24. F. Galton, Inquiries into human faculty and its development, London (1883).
25. T.H. Huxley, 'Evolution and ethics', in Huxley, Evolution and ethics, London (1906).
26. F. Galton, Hereditary genius, 2nd edn. London, (1892), 360.
27. See e.g. J.D.Y. Peel, Herbert Spencer: the evolution of a sociologist, London, (1971).
28. In the draft of her Ph.D. thesis. I mention the point as I find myself in agreement, but do not claim it as original.
29. Ibid. Cowan found from conversations with Galton's collateral descendants that he was a staunch supporter of Lord Salisbury. Forrest's work supports this interpretation of Galton's political allegiances.
30. F. Galton, op.cit. (note 26), 348.
31. For this, see Forrest op.cit (note 4), chapter 7.
32. See, for example. A. Quetelet, Sur l'homme et le développement de ses facultés, essai d'une physique sociale, Paris (1835).
33. A typical example of the literature available is G.B. Airy, On the algebraical and numerical theory of errors of observation, London (1861). Galton owned a copy of this which he annotated heavily.

34. For a general account of statistical societies and their activities, see P. Abrams, The origins of British sociology, 1834-1914, Chicago (1968). See also F.J. Mouat's 'History of the Statistical Society of London' in the Jubilee Volume of the Statistical Society, London (1885).
See also M. Cullen, The statistical movement in early Victorian Britain: the foundations of empirical social research, Sussex (1975).
35. V. Hilts, 'Statistics and social science', in eds. R. Giere and S. Westfall, Foundations of scientific method in the nineteenth century, Bloomington (1973), 243-258.
36. F. Galton, op.cit. (note 5), 305.
37. F. Galton, Natural inheritance, London (1889), 66.
38. Galton's main findings were recorded in op.cit (note 37).
39. For a modern account, see C.E. Weatherburn, A first course in mathematical statistics, Cambridge (1968) See Chapter 4, 'Bivariate distributions. Regression and correlation'.
40. See Ruth Cowan, 'Francis Galton's statistical ideas: the influence of eugenics,' Isis, 63 (1972), 509-528.
See also, F. Galton, op.cit (note 5), 251.
41. F. Galton, Presidential address, Jnl. Royal Anthropol. Inst., 19, (1888) 401-419.
42. F. Galton. 'On co-relations and their measurement, chiefly from anthropometric data', Proc.Roy.Soc. 45, (1888), 135-145.
43. Ibid. For further explanation, see Weatherburn, op.cit. (note 39).
44. It is a simple consequence of equation (i) above.
45. See Charles Darwin, Variation of animals and plants under domestication 2 vols, London (1868).
See also G.L. Geison, 'Darwin and heredity: the evolution of his hypothesis of pangenesis', Jnl.hist. of medicine, 24 (1969) 375-411.
46. There are many accounts of Weisman's ideas available, but a particularly accessible one is to be found in J.A. Thomson, Heredity, 1908. See chapter 12.
47. See, for example. Edward J. Pfeffer, 'The genesis of American neo-Lamarckism', Isis, 56 (1965), 156-167.
48. See Herbert Spencer, Principles of biology, 2nd.edn., London (1898), vol.1, 650.

49. For an account of this episode, see Pearson, op.cit. (note 1). vol 2, 156-183.
50. A typical presentation of the thesis was made in F. Galton, 'A theory of heredity', Contemporary review, 27, (1875) 325-338.
51. A.R. Wallace, My life, 2 vols, London (1905). See vol 2, 21-22.
52. The invalidity was pointed up in R.G. Swinburne, 'Galton's law - formulation and development', Annals of Science, 21 (1965), 15-31. Swinburne does not point up the oddity of the dual reference of the law.
53. F. Galton, Natural inheritance, London (1889), 136.
54. Ibid., see p.188. See also J.F. Wilkie, 'Galton's contribution to the theory of evolution with special reference to his use of models and metaphors', Annals of science, 11 (1955), 194-205.
55. F. Galton, Hereditary genius, London (1869). See the chapter on the 'Comparative worth of different races'.
56. Ibid, 2nd edn. (1892), see p 33.
57. F. Galton, op.cit. (note 24), 19.
58. Ibid, 23.
59. Ibid, 19.
60. Jnl.anthrop.institute, 14, (1884), 205-218

61. See e.g. J.McKeen Cattell, 'Mental tests and measurements', Mind, 15 (1890), 373-381.
62. A 'Key' to this work has recently been published by Victor Hilts in Transactions of the American Phil.Soc. 65 (part 5), (1975). The work identifies the scientists whose words are anonymously reproduced in English men.
63. A. de Candolle, Histoire des sciences et des savants depuis deux siècles, Geneva (1873).
64. Galton, op.cit (in text).
65. Galton, op.cit. (note 24) 156.
66. Ibid, 172.
67. In Mallock's work, London (1877) 'Storks' is Huxley, 'Stockton' is Tyndall and 'Saunders' is Clifford.

68. Kept, in part, in the Galton archive, University College London.
69. In discussion.
70. F.Y. Edgeworth, review in Nature, 39, (1889). 603-604.
71. The copy is kept in the office of the Department of Statistics and computer science, University College London.
72. For an account, see Pearson, op.cit. (note 1), vol 2, 416-424.
73. Pearson to Galton Jan 10, 1901. Pearson archive, University College London.
74. F. Galton, 'The possible improvement of the human breed, under the existing conditions of law and sentiment', reprinted in F. Galton, Essays in eugenics, London (1909).
75. Ibid. See also V. Hiltz, 'William Farr (1807-1883) and the human unit', Victorian studies, 14, (1970). 143-150.
76. F. Galton, 'Eugenics: its definition, scope and aims', Sociological papers (1904), 45-50. Responses to Galton's paper follow immediately thereupon.
77. F. Galton, Sociological papers, vol 2, (1905), 1-53.
78. F. Galton, 'Probability, the foundation of eugenics', reproduced in F. Galton, Essays in eugenics, London, Eugenics Education Society (1909), 73-99.
79. See Forrest, op.cit. (note 4), 260.
80. Schuster, however, had not lost his enthusiasm for eugenics, and went on to write a standard text on eugenics. See Edgar Schuster, Eugenics, London (1912).
81. See K. Pearson op.cit. (footnote 1), vol 3a ,300

82. This was C.D. Davenport's Eugenics record office set up at Cold Spring Harbor with funds provided by Mrs. Harriman. For details, see M.H. Haller, Eugenics: Hereditarian attitudes in American thought. New Jersey, (1963). See chapter 5 'Genetics and eugenics'.
83. See Forrest, op.cit (note 4), 270.
84. Pearson's Department of applied mathematics received a grant of money from the Drapers Company from 1903 to 1925 at the rate of £500 pa. For details see, Lyndsay Farrall, The English eugenics movement, 1865-1925, (unpublished doctoral thesis of Indiana University, 1969), 136.

85. For an account of this, see G.R. Searle, Eugenics and politics in Britain 1900-1914, Leyden (1976). See chapter²_^ 'The development of a eugenics movement in Britain'.
86. The consequent upon the furore raised by the publication of Kamin, op.cit (note 3, chapter I).
See for example the article by Oliver Gillie on P.1 of the Sunday Times, October 24, 1976.
87. F. Galton op.cit (note 56), 12.

Chapter 3 The biometric school and the profession of statistics.

Introduction

W.F.R.Weldon

Karl Pearson

Cambridge to London
Pre-biometric London
Biometric London
Eugenic London

F.Y.Edgeworth

The Biometric School and:

G.U.Yule
W.S.Gossett
Major Greenwood
R.A.Fisher
Other workers (Laski, Carr-Saunders, Pearl, Neyman and others.)

Introduction

In this chapter I commence the task of describing and explaining the exfoliation of the Galtonian tradition, focusing, very properly, upon Galton's most influential follower, Karl Pearson, and upon Pearson's development along with the biologist Weldon, of a 'Biometric School' of mathematical biology in London in the 1890's. The importance of these events can hardly be overstated, as the Biometric School, centred upon University College London, was to be the embryo from which the modern discipline of and profession of statistics was to grow. Pearson's methods and ideas, by turn, were to assist enormously in the development of the other areas of endeavour described in the Introduction. This chapter focuses less upon explanation than upon chronology and historical description, leaving the task of analysis and explanation to the chapter that follows immediately.

W.F.R. Weldon (1860 - 1906)

We do well to commence by looking at the career of W.F.R. Weldon¹ the zoologist, who was to meet with Pearson and stimulate him into turning his mathematical talents upon certain biological problems, thereby making massive theoretical and institutional developments in statistics. W.F.R. Weldon was the son of Walter Weldon the industrial chemist. Walter made a fortune from chemistry, and passed a great deal on to his son. He, by turn, harboured his resources - for his widow was able to donate paintings by Corot, Sisley, Blake and others to the Ashmolean Museum and to leave the great part of her fortune of £68,000 to endow a chair of biometry at University College London.² The first incumbent was J.B.S. Haldane.

Weldon's youth was spent in London suburbs, and, thereafter, in Cambridge where, as a student at St John's College, he read for the Natural Sciences Tripos, concentrating upon biological subjects. At Cambridge a renaissance was taking place in British biology. Alfred Newton³ and C.C. Babington⁴ were respectively professors of zoology and botany, but, in

their hearts and in their practices, they remained close to the old 'parson-naturalist' tradition of British natural history, close, that is, to a tradition that stressed systematics, egg collecting and bird stuffing. But, a new generation of biological scientists was emerging, taking their lead in many cases from Michael Foster (1836 - 1907), appointed praelector in physiology at Trinity College in 1870. His 'new men' began to come to the fore in the mid and late 70's. The general atmosphere of change is nicely caught in a letter from A.G. Tansley to J.R. Baker, in which Tansley, the eminent botanist, described developments as he saw them. Naturally, Tansley stresses the botanical side of things.⁵

The group of great German botanists who flourished in the middle of the last century - I mean men like Hugo von Mohl, Sanio, Hofmeister, Alex Braun, Nageli, and a little later, Sachs and Pfeffer, may be said to have founded the new 'wissenschaftliche Botanik' in contradistinction to the old 'systematische Botanik'. That was an exact parallel with what you mean by 'General Zoology', though it included plant anatomy. In England, very little was known about it, and it was not until the seventies, and particularly the eighties that English university students began to go to Germany to work in the laboratories of the masters and their followers because the professors in England (with a few exceptions) knew little or nothing about it. When they came back they began to teach and research along the same lines. Meanwhile, the old systematic tradition, mainly concerned with flowering plants, was still strong. C.C. Babington was still professor of botany at Cambridge when I was an undergraduate in 1890-93, and he knew nothing about 'wissenschaftliche Botanik'. That was taught by Francis Darwin and Walter Gardiner, who had worked in Germany. After that, the old systematic tradition was carried on mainly by amateurs...

In zoology, inspiration also came from Germany, and from Englishmen who had trained with German professors. In zoology, which was Weldon's favoured subject, the coming men at Cambridge in the late 70's and early 80's were Francis M. Balfour (1851-1882) and his assistant Adam Sedgwick (1854 - 1913). Balfour, by turn, was the student of Foster, founder of the Cambridge school of physiology, and, like his friend and mentor T.H. Huxley, a pioneer of laboratory tuition in British universities.⁶ Like most Cambridge zoologists of his period, Balfour was a morphologist, working mainly in the area of comparative embryology. He produced the famous Treatise on comparative embryology (1880-1881). This led to the creation of a special chair in animal morphology at Cambridge for Balfour who was appointed in 1882, but, shortly afterwards, fell to his death in a climbing accident.

Thereafter, Sedgwick took up the intellectual leadership in Cambridge Zoology, and continued to initiate students into the practices of morphological research.⁷ This, it may be noted, was a style that did not seek merely to establish the detailed structures of the different forms of life extant and extinct, but also to establish the history of evolution, to show the phylogenetic connections between the different species. In short, a major goal for Cambridge zoologists was the establishment of historical, evolutionary relationships which existed between various groups of species, extant and extinct. Here, in fact, is one of the rationales for Balfour's preoccupation with embryology. For, as the fossil record was far from complete, the establishment of phylogenetic relations involved a degree of theorising. And, generally speaking, the form of theorising most favoured at Cambridge was one based upon embryology taken in conjunction with some variant of Haeckel's famous dictum that ontogeny (i.e., the pattern of embryological development found in the individual member of a species) recapitulates phylogeny (i.e., the evolutionary history of the species itself). Balfour, for example, was most impressed by the potentialities of such a principle. He, like Sedgwick, saw himself as expanding Darwin's great enterprise by tracing out the particular patterns of change which had been enacted on Earth during the history of life. Like Sedgwick and many others working in the morphological 'paradigm', Balfour was not so much interested in the mechanisms whereby these patterns had been brought about. There was very little work done in Cambridge, during the whole 19th century, on the processes of variation and heredity.⁸

Weldon graduated, with first class honours in 1881. He stayed on at Cambridge working on morphological problems and, in 1884, was rewarded by being appointed to a new University lectureship in the advanced morphology of invertebrates. He was a fortunate man, as tenured academic posts were particularly hard to come by at that time. In 1884, for example, Cambridge had only 3 ^{University} appointments in Zoology at or above the lecturer level. Weldon stayed at Cambridge until 1890, when he was able to move to University College London to take up the Jodrell chair of zoology and comparative anatomy in succession to E. Ray Lankester.⁹ The grandeur of the appointment should not be overstressed, for, at University College, the provision for zoology was less than at Cambridge.¹⁰

Whilst at Cambridge, Weldon began to experience doubts about the intellectual fecundity of the morphological tradition in which he had been raised, and began to look about him for new and more effective methods for thinking about evolution. He considered that there might be an avenue of advance within Galton's Natural inheritance, published in 1889, and a work that reviewed all of Galton's various statistical investigations of human heredity and variation.

Weldon immediately began to apply Galton's techniques to the study of wild populations of crustacea, and was able to obtain assistance from Galton himself. Possibly the meeting was brought about via Galton's refereeing Weldon's early papers, for there is a letter extant,¹¹ dated 7th January 1890, in which Weldon writes to Galton referring to his 'condemnation of my papers to the Royal Society' and expressing a desire to 'learn the nature of my triple misconception as soon as possible'. Clearly, things were sorted out, as Weldon began to publish papers in the Proceedings of the Royal Society in which he gave the results obtained by carrying out statistical surveys of various populations - papers in which he showed that normally distributed variations seemed to prevail in the wild as well as in man, papers in which he investigated the values of correlations between pairs of organs (eg, carapace length and tergum length in the shrimp) in a number of local races, with the hope of establishing a new numerical taxonomy capable of giving a guide to evolutionary relationships. This latter proposal did not long survive.

The essence of Weldon's early research programme is clearly laid out in a paper of 1893, where Weldon made a declaration seen by Pearson as 'speech-making' and as having formulated the 'fundamental principles of biometry'. We shall return to this statement.¹²

It cannot be too strongly urged that the problems of animal evolution is essentially a statistical problem: that before we can properly estimate the changes at present going on in a race or species we must know accurately (a) the percentage of animals which exhibit a given amount of abnormality with regard to a particular character; (b) the degree of abnormality of other organs which accompanies a given abnormality of one; (c) the difference between the death rate per cent, in animals of different degrees of abnormality with respect to any organ; (d) the abnormality of offspring in terms of the abnormality of parents and vice versa. These are all questions of arithmetic; and when we know the numerical answers to these questions for a number of species we shall know the deviation and the rate of change in these species at the present day - a knowledge which is the only legitimate basis for speculations as to their past history, and future fate.

The statement may be unclear, but, for its period, it was totally different, totally unlike any other methodological declaration to be found in British biology. Clearly, the research programme outlined here required statistical and mathematical sophistication - more so than Galton could command. Weldon had to seek mathematical assistance from elsewhere - notably from MacAlister of Cambridge. But, as might perhaps be expected, given the contemporary nature of mathematical tuition and research at Cambridge, assistance of the required sort was not easy to come by. Other sources would have to be investigated.

One such source was immediately to hand in the form of Karl Pearson (1857-1936), who had been professor of applied mathematics at University College since 1884. He and Weldon were soon united by their common interest in academic politics, which were rife in the University of London in the late 80's. Weldon's predecessor in the Jodrell chair, E. Ray Lankester, had been prominent in the reform movement, in the 'Association for the promotion of a teaching University for London', set up in 1884,¹³ and Weldon continued in this tradition, which contained Karl Pearson. Pearson, Weldon and Carey Foster soon joined to form an 'Association for promoting a professorial University in London', a ginger group of London academics urging power to the professors. Its aim was expressed thus¹

The creation of a homogeneous academic body with power to absorb, not to federate existing institutions of academic rank, seems the real solution of the problem. An academic body of this character might well be organised so far as teaching is concerned on the broad lines of a Scottish University. Such a corporation may be conveniently spoken of as a professorial university to distinguish it from a collegiate or federal university.

T.H. Huxley was persuaded to act as president, but was seen by Pearson as something of a trimmer, as a pragmatist rather than a true follower. Pearson resigned his secretaryship in a strongly worded letter to the Times; Huxley replied¹⁵ with words of rebuke for 'one-eyed fanatics, ignorant of the commonest conventions of official relations, and content with nothing if they cannot get everything their own way'. Pearson, naturally, saw things differently, and was to claim that, though Weldon took up his (Pearson's) secretaryship, the actual reorganisation of the University, achieved before the end of the century, was bitterly resented by Weldon.¹⁶

It is hard to say which came first in Weldon's and Pearson's relationship - academic politics or biometrical consultations. Certainly, extant letters suggest that it was the former. The first extant letter from Weldon to Pearson dated 5 March 1892 informs the latter that¹⁷

I send you the protest, which is already distributed
(by 3.0 am collection today)

I addressed with my own hand 320 envelopes
last night: so cannot write much today.

Weldon, I assume, was referring to a protest against the Albert Charter, a scheme - that was to fail - to unite University and King's College into an Albert University of London.

No extant letters on biometric topics date from before 27 November 1892, when Weldon wrote discussing the problem of resolving an observed asymmetrical distribution of crab-dimensions into the sum of two normal ones. This letter marks the onset of a vast correspondence on biometric topics, and of a friendship that was to survive the strain put upon it by Pearson's criticism of Huxley, with whom Weldon identified most strongly.

The correspondence of November 1892 led directly to Pearson's first contribution to biometry - to his essay on the analysis of asymmetric frequency curves into Gaussian components. (See Fig I and caption).¹⁸ This was published in the Philosophical Transactions of the Royal Society in 1893 as the first of a long series of contributions to 'the mathematical theory of evolution'. Thereafter, Pearson was quite caught up in matters biometric, publishing paper after paper on mathematical evolution. The significance of these we shall shortly see.

From an early stage, Weldon was committed to the view that evolutionary change was generally a continuous and gradual process, brought about by the operation of natural selection upon the small variations displayed by all the members of large intercrossing populations. In this respect, his thought was attuned to Darwin's, who, especially in his later writings, went out of his way to stress the gradualness and continuity of evolutionary change. For Darwin, the advocate of discontinuity was faced with many problems¹⁹

He will be further compelled to believe that many structures beautifully adapted to all the other parts of the same creature and to the surrounding conditions, have been suddenly produced; and of such complex and wonderful co-adaptations, he will not be able to assign a shadow of explanation. To admit all this is, as it seems to me, to enter into the realms of miracle, and to leave those of science.

Image removed due to third party copyright.

An insistence on gradual evolutionary change was common among late 19th century Darwinians, many of whom expended considerable energy in the quest of showing the utility of apparently inutile characters. On the other hand, the advocates of continuity did not go completely unchallenged. Huxley himself had always insisted that Darwin did ill to rule out the possibility of saltatory evolution,²⁰ and Galton himself, as mentioned, was similarly an advocate of saltatory evolution.

Soon, Weldon found himself and his biometric enterprise opposed by an advocate of ^{dis}continuity, in the shape of William Bateson (1861-1926). Bateson had graduated with first class honours in the Cambridge natural sciences tripos in the year after Weldon's graduation, (i.e. 1882) and had gone on to a fellowship of St. John's College and to a considerable reputation as a rising star in zoological circles.²¹

In the mid and late 1890's, Weldon continued his statistical analyses of wild populations of crustacea, sometimes with the assistance of a Royal Society 'Evolution Committee', which he and Galton caused to come into being in 1893.²² During this period, Weldon's most notable work was a series of observations which he interpreted²³ as showing the existence of a selective death rate amongst Plymouth crabs - a selective death rate with respect to a normally distributed bodily dimension, 'frontal breadth'. Interestingly, he claimed to have found a stabilising selection - i.e., one that selectively eliminated the more extreme variations on both sides of the mean 'frontal breadth'. This, I hope, is made clear in the accompanying figure 2 and caption. Weldon was keen to promote his findings as a vindication for the Darwinian viewpoint in evolution, and this interpretation of his findings drew him deeper into conflict with his former colleague Bateson, who, as noted, was an advocate of the discontinuist position. At the same time, most interestingly, Weldon abandoned one of Darwin's intellectual stances, namely that of discussing 'complex and wonderful co-adaptations'. Weldon thought that the arrival of statistical methods made such discussions redundant. For, we find him writing that²⁴

Knowing that a given deviation from the mean character is associated with a greater or less percentage death rate in the animals possessing it, the importance of such a deviation can be estimated without the necessity of inquiring how that increase or decrease in the death rate is brought about, so that all ideas of 'functional adaptation' become unnecessary.

Such a perspective was deeply repellant to Bateson, who was led by it to become even more distrustful of Weldon, and of his and Pearson's new

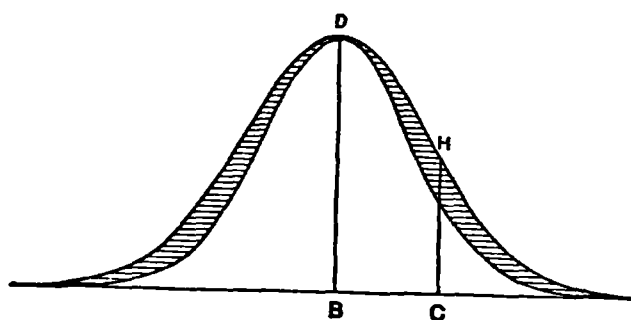


Fig.2. Based on Weldon's 1895 paper on selective death rate in crabs. Weldon claimed that the difference between the inner and the outer curves represented the fraction of a crab population destroyed by natural selection. This, he claimed, was a first statistical demonstration of the operation of natural selection. For further details, see Weldon, op.cit. (note 23, chapter 3).

74

'biometric' methods. In this growing disagreement, we have the origins of a most famous scientific controversy - namely the so-called 'biometric-Mendelian' debate which would reach its climax in the early years of the 20th century, when Bateson and his followers adopted the ideas of Gregor Mendel, and were opposed by Weldon and Pearson with considerable vehemence. A whole chapter is devoted to this debate, and so there is no point in continuing the discussion of it here.

More points on Weldon will emerge in the discussion of the biometric - Mendelian debate, and in the remarks on Pearson which follow. But, for the present, it is worth noting that Weldon moved to Oxford in 1900 to take up the chair of zoology vacated by Ray Lankester's removal to the British Museum. But, once again, his close connection with Pearson did not falter. Indeed, it was, if anything, more strengthened by their fight against Mendelism, and by their decision to found a new journal in which to develop their new 'biometric' approach to the study of life. This, of course, was Biometrika, founded in 1901 after Pearson had experienced difficulty with the biological members of the Royal Society, whom, he felt, reacted adversely to his work as they had no conception of the importance of the numerical methods that he and Weldon were developing.²⁵ Thus came into existence what is now a foremost journal for professional statisticians, and which, until 1948, bore the legend 'A journal for the statistical study of biological problems' : by 1948, however, the biological content had long been more fictional than real, an indication of the way in which statistics soon separated itself from its biometric integuments.

Weldon died in 1906, from a sudden attack of pneumonia - though it is said that his strength had been gravely weakened by his continuous work, devoted largely, in his final years, to the onslaught upon the developing discipline of Mendelian genetics.

Karl Pearson (1857-1936)

In Pearson²⁶ we have a man of much greater achievement than Weldon, and, quite rightly, he is the central figure in this study. But, just as Sherlock Holmes depended upon Watson whilst at the same time surpassing him in investigatory skills, so did Pearson depend upon Weldon whilst at the same time surpassing him. Meeting with Weldon was a matter of great importance to Pearson's development. Certainly, it is hard not to agree with the assessment of J.B.S. Haldane when he wrote that a frank evaluation of Pearson, made in 1890, would have run as follows.²⁷

He is a first rate teacher of applied mathematics, and a scholarly compiler of the work of more original men. He has a knowledge of literature and art most unusual in a professor of mathematics. He is somewhat of a radical, but he is only thirty three years old. He will settle down as a respectable and useful member of society, and may expect a knighthood if he survives to sixty. He will never produce work of great originality, but the College need not be sorry to have appointed him.

Pearson, at the time of his meeting with Weldon, had experienced an intellectual and personal development which must frequently have led him to reflect that behind many silver linings there is a dark cloud. Attempts to become a lawyer, a philosopher, a poet and a mathematical physicist had met with results ranging from good to disastrous. Most of his triumphs lay in the future.

Pearson was born in 1857, the younger son of William and Fanny Pearson. William was a fine example of early Victorian social mobility, a man who rose from a rural background, via hard work, intelligence and the odd stroke of luck, to become established as a successful London barrister. Fanny was the daughter of a ship's captain and owner.

I return to Pearson's childhood in the chapter following - and it was a bleak travail. The father was strong and dominating, the mother soft, self-indulgent and extremely cloying. Pearson was educated at University College School in London, though his elder brother, Arthur, who seems to have come to a possibly disreputable end before Pearson's thirtieth year, was sent to Rugby.

An autobiographical note due to William Pearson (1822-1907) runs as follows²⁸

When I was in my 18th year I left home for Edinburgh where I remained over six years. My schooling had begun at the village dame school when I learnt my a,b,c. The village curate took a fancy to me, and I took a fancy to hounding him with impish tricks. However, he was very patient with me, dear fellow, and at length succeeding in driving a little learning and some decent behaviour into my stupid noddle. Then I had my teaching at schools at Easingwold and Malton. After passing through the classes at Edinburgh University I obtained through Lord Carlisle an appointment in H.M.'s Office of Woods and Forests, but my rebellious spirit could not brook the 10 to 4 slavery and so I abandon that and came to the bar. When I made a precarious income of £250 a year there I fancied myself rich and independent and after the manner of my kind, took to marrying - the rest you know.

William was excellently suited to the role of stern Victorian paterfamilias, and his letters depict him as always ready with some moral advice or some scheme for his son's advancement. It is said that exemplum docet, whereas exempla obscurant, and so I will give just one example of his style of letter-writing. The extract below is taken from a letter to Karl, dated 1st. October, 1873.²⁹

I know what the first starting from home to begin the battle of life is. But cheer up old boy you will soon shake down and settle down, and although you will never find elsewhere the tender loving kindness of home you may not after a little time find your associates such bad fellows after all. Be affable and friendly with them and you will extract their better qualities while you do not imitate or follow their failings or their faults. They will soon learn to respect you. Remember also this is the kind of world we have to fight our battle of life in, and it is better to begin in time and get used to the world before we have to be dependent on it, than to plunge into the fight at once. You are now beginning to form your future character and to pass through fire - the fire that is to make you better and wiser than before.

Fanny, by contrast, seems to have tended to hypochondria, and, clearly, found William something of a trial. She did not hesitate to unburden herself upon her growing son, as, for example, on the 21st October, 1873, when she wrote to Karl that,³⁰

Your letter and card were very welcome to your lonely mother, for though Papa remains longer in the dining room after dinner he is intensely dull, never speaking unless I call it forth. I can so well sympathise with your feeling that you are not working enough, you know that is the only means for you to get on in life, but don't my darling be discouraged by the inaction of others, only a very short space of your life is to be passed there. Do the best you can for yourself, knowing your own future will depend so much on yourself.

At sixteen, Pearson was removed from school and sent to a crammer at Hitchin, where, understandably, he was malcontent and where he loathed his loutish companions. He was the subject of regular missives from home. His father was preoccupied with the need for academic success, telling his son that it could bring preferment at the bar. Letter after letter discussed Cambridge, which college, what scholarship - and so on. Pearson junior, it was planned, would read for the mathematics tripos, still, in the late 70's, the most prestigious.

Karl spent only one year at Hitchin. In 1874 he went to Cambridge to be tutored by the great Routh, and, in 1875, he obtained a scholarship at King's College, then small and overwhelmingly Etonian.³¹ In late life, Pearson reflected on the delights of a mathematical education at Cambridge, but, after his degree he showed no keenness for the place at all. He did enjoy some of the company - possibly, at times, that of Oscar Browning the historian, and certainly that of Henry Bradshaw^{the librarian} whom he saw as a sympathetic and wise father figure. Certainly when, later on, Pearson fell in with Galton, he paid him the ultimate accolade of comparison with Bradshaw. Both men, said Pearson, filled his need for the guidance of an older and a wiser hand.

At King's, Pearson showed courage in bearding the authorities over compulsory divinity lectures and chapel attendance. These he refused, and with some assistance from his father, got away with his refusal.³²

As we shall see, his circle of friends at King's was ^bprobably small. And, among the students, the closest was Robert Parker (1857-1918), future law lord and father of Lord Chief Justice Parker.³³ He was also close with 'Josh' Conway, the future Lord Conway, and with the mathematician W.H. Macaulay.

Pearson graduated in 1879, emerging as third wrangler - that is to say, as third man overall in the mathematics tripos for that year. This was a tremendous accomplishment, and a valuable one. Graduates of the Cambridge tripos were highly prized, and the highest wranglers were thought worthy of newspaper write-ups.

The next five years of Pearson's life were crucial. In 1880 he was made a fellow of King's - an appointment which gave him six years of financial freedom, time in which to develop his mind and to decide what he would do in life. Immediately after graduating, he went to Germany, studying in Heidelberg and in Berlin. By 1879, he had already acquired a taste for German literature - particularly for Goethe. He studied physics

and philosophy in Heidelberg, and thereafter law in Berlin. On his return to England he took up a legal career, and was called to the bar in 1881. His next major appointment came in 1884 when, after several attempts to obtain mathematical posts elsewhere, he was made professor of applied mathematics at University College London, the chair having fallen vacant. Pearson stayed at University College for the rest of his working days, though this was not entirely of his own choosing, for he made various applications for other posts, notably for the Savilian professorship of geometry in Oxford. Certainly, his life was to be

transformed by his meeting with Weldon in 1890, and by their joint development of the would-be science of biometry. For, it is on account of biometry and the statistical theory and institutions which it generated that Pearson is principally famous. The matter has been put very precisely by the American statistician and historian Churchill Eisenhart, whose words are therefore reproduced below.³⁴

Pearson communicated some thirty-five papers on statistical matters to the Royal Society during 1893-1901. By 1906 he had published over seventy additional papers embodying further statistical theory and applications. In retrospect, it is clear that Pearson's contributions during this period firmly established statistics as a discipline in its own right. Yet, at the time, "the main purpose of all this work" was not development of statistical theory and techniques for their own sake but, rather, "development and application of statistical methods for the study of problems of heredity and evolution".

The developments that these led to have also been neatly encapsulated by another author, S. Stouffer the sociologist, in an article recalling a youthful visit to Pearson's laboratory. The tenor of Stouffer's work is quite as interesting as its content.³⁵

I wish I could communicate to you, and especially to those of you who are just now beginning your professional careers in a world of Statistics incredibly more sophisticated than that of Karl Pearson's day, something of the thrill in meeting in person and studying under a man of Pearson's immense reputation. Author of the Grammar of Science; perfecter of simple linear correlation; inventor of multiple and partial correlation, of curvilinear correlation, of tetrachoric and bi-serial correlation; discoverer of the χ^2 -function for summarizing multinomial data with magnificent simplicity; builder of a beautiful system of frequency curves derived from a single differential equation which in turn harked back to the hypergeometric series; founder of Biometrik and author or co-author of a prolific literature applying these new statistics to biological and sociological data - Karl Pearson was a hero of Asgard to an American boy vouchsafed a visit to the home of the gods. Indeed, Pearson was Thor himself - for the thunderbolts with which he attacked unsparingly those who dared oppose him still were echoing and reechoing. His battles to defend Galton's Law of Ancestral Inheritance against the heresy of rediscovered Mendelism may have faded somewhat in the past, but his fulmination against those who questioned his faith in nature as against nurture and dared to challenge voluminous correlational data which he and his co-workers had amassed were still audible, and the air was now electric with conflict as he stubbornly defended his statistical system against the threats of the Fisherian revolution.

As we have seen, these intellectual developments were accompanied by concrete ones, sometimes literally so, as when new buildings for Pearson's Department of applied statistics was opened by the Minister of Health in 1920. The transformation of Pearson's life which was begun by the meeting with Weldon was completed by his subsequent friendship with Galton, by his achieving external funding for biometry in 1902, by his taking command of the Galton Laboratory in 1906, and by the transfer to the new Galton chair of eugenics in 1911, about which Pearson set up his department of applied statistics - home of Biometrika and, later on, the Annals of eugenics. On his retirement in 1933, Pearson's chair went to R.A. Fisher, and a new chair of statistics was created for his son E.S. Pearson, collaborator with Jerzy Neyman in the production of Neyman-Pearson statistics. In 1937, as noted, the Weldon chair of biometry was created, and its first incumbent was J.B.S. Haldane.

What happened during these years? We are well advised to adopt E.S. Pearson's strategy of dividing the life into periods.³⁶ The first of these is the period from 1879 to 1884 - from success at Cambridge to appointment at University College London. Once again I should add that this chapter concentrates more on the chronicling of events and the posing of questions than on the explanation of the events and the resolution of those problems. Those items are the business of subsequent chapters.

Cambridge to London

The basic form of Pearson's life in this period is easy to establish. He spent parts of 1879 and 1880 as a student in Germany. He was made a fellow of King's College Cambridge in 1880, and, when not in Germany, was officially apprenticed a lawyer. He took rooms in Harcourt Buildings, Inner Temple, in November 1880, read in chambers in Lincoln's Inn, was called to the bar in 1881 and took the degree of Ll. B. Ostensibly Pearson was forging a legal career, thereby following many high wranglers in the Cambridge mathematics tripos. Then, as now, there was a good living to be made at the bar.

In fact, Pearson was never much concerned to make money and his correspondence shows that he cordially loathed the law. An illuminating letter passed between him and Robert Parker on 24 October 1881. The following passage is an extract from that letter.³⁷

I went from there home. Told my Governor that Macrory the patent man thought that it would be better I should not enter his chambers at present (how I rejoiced in that reprival three days ago!) and that I ought to go to an equity pleader. The Governor remarked that it was high time I did: something as three years were already gone and I should want another six before I did anything at the bar, that I must really take to reading law and only reading it. That he would find a pleader at once, but that I had better spend my time in reading criminal law, which was a most interesting science. God! interesting science! - rape, murder and the pettiest of thefts can form an interesting science! How I wished he had been a person to whom I could open my heart. I suppose there are such parents. If it wasn't for my mother I think I should realise my old idea of the back-woods and join Patrick in Ohio. I must have fresh air and I cannot get it here.

Six years of criminal law. I wonder what one will be like at 30? Yours K.P.

The law, in short, was a mind-numbing bore. Pearson was keen to get out at almost any cost; any cost that is, short of returning to work at King's College. This is shown by several job applications and adjunct correspondence. A letter from G.W. Prothero of King's, dated January 8th 1883 shows that Pearson was thinking of a commercial career. This letter and others show that Pearson was wanted at King's, but would not come. Prothero remarks that the 'spirit of the place is in many respects probably quite as alien to me as it is to you'.³⁸

My dear Pearson,

I have just heard from R.H. Macaulay that J. Bryce, who wrote to me about University men for their business, is at present at Rangoon. His address in "The Bombay and Burmah Trading Corporation Ltd., Rangoon". He went out last October, and is expected back in May. If you like I will write at once to him, and will do so with pleasure - so don't scruple to ask. Or if you like we can wait till he comes home. Unfortunately I know none of the other partners.

There is another business-house in the City, a South-American business I believe, headed by one Burke, an Oxford first-class man - which young Birch of this college has just gone into. But it is a small house, and Birch says they want men of 22 or so, not older. Still they might be willing, and I will ask if you like.

Are you too old for any of the Civil Service appointments? If not, I should have thought they would suit you better than business.

I wish I could persuade you to come here - there is an opening for you here, and what is more there is something for you to help in making - that is a new college, out of this old one. The spirit of the place is in many respects probably quite as alien to me as it is to you, and that is just why I want you to come - that you may introduce another element. The spirit of a place is not permanent, and you supply just the difference I want to see here. The college sticks in the mud now, and it has the makings of a big and noble place, and I want to see it got out of its ruts. Why don't you come and help? There is actual teaching work to be done. Frost will do very little more work now, and you can take his place - that gives you a footing, the rest you can make for yourself. There is a future for the Universities and for this college if men like you will come and work at it. As to our qualifications, you needn't suppose that a college Don is such a very different person from the rest of the world, or at any rate need be - that notion savours of your early undergraduate days (forgive me), and in some ways you are qualified very thoroughly for the post.

I wish to goodness you would think about it seriously - and let us talk over it when you next come up, now in the midst of nest-building.

In the same year (30 July) we find that Pearson has applied for the principa/ship of Firth College, Sheffield. A letter of recommendation to Henry Bradshaw, tells the same story. Pearson could work in Cambridge if he desired, but is antagonistic to its conservatism.³⁹

Mr Pearson is a fellow of my own College and I would give anything to induce him to come up and take University and College work, so as to help our College to take its place and to hold its own place in the University. If he had a distinct bent for the bar, or if he could be at the head of a place like Firth College, I should not grudge losing him because he would then have scope for his powers. But if he came here there would be all the antiquated traditions of an old place like this mixed up with the good traditions and above all there would be the workers and upholders of these antiquated traditions to fight against and all this implies great waste of powers and energies at a time that it is quite possible to find a sphere of work free from such friction. In a word some men are born heads and some are born subordinates. Some men will be rebels as subordinates while they would be admirable as heads. Mr Pearson is to my mind one of these.

If Cambridge and the law repelled, then other items had the power to attract

During this period, Pearson was close to three Cambridge friends W.M. Conway, Robert Parker, and, to a lesser degree, W. Macaulay the mathematician. And like some of his fellows, he was doing some University extension lectures. In the spring of 1882, he gave a course of lectures on 'German social life

and thought' in Blackheath, in which he traced 'the rise and fall of the successive German Ideals from the earliest times up to 1500, which period embraces the growth and decay of the principle of unity in Germany, as embodied in the Holy Roman Empire and in the Holy Catholic Church'. In the Autumn, he offered a course of lectures on 'the history of the German folk (1500-1550) "The reformation"'.

It was not only history that interested Pearson in this period, for he was attempting to build reputations as radical social critic and as a man of letters. It is no surprise to find that he called upon Karl Marx in 1881. Nor, when his views have been discussed, will it seem surprising that Marx and Pearson seem not to have seen eye to eye. For, on 13 March 1881, Pearson wrote to Parker, telling him that 'Marx wrote me a somewhat sarcastic note with which ends our connection'.⁴⁰

What can be said about Pearson's development in this period up till his appointment at University College? Well, we can see him trying to develop along three main lines - mathematical, literary and philosophical and social.

On the mathematical front, Pearson published very little - one paper in 1879, another in 1880, and two more in 1883. At the time of his appointment at University College, he had not made great published contributions to mathematics.

More effort, in fact, had been given to literary and philosophical pursuits. Taking these by turn, we find that Pearson published The New Werther in 1880, under the pen-name 'Loki', a 'Farewell to Cambridge', in verse, in 1881, and, most notably, The trinity. A nineteenth century passion play in 1882.

The New Werther, literarily speaking, is an abortion, a poor imitation of Goethe's original, telling in turgid style, the tale of a British idealist studying in Germany, who befriends a Jew, Raphael, and speaks with him of literature, philosophy and social democracy. Arthur, the tragic hero, introduces Raphael to his girl-friend, Ethel. Inevitably, Raphael and Ethel become mutually enamoured, and the desolate and self-pitying Arthur commits suicide, though not before putting down his dog Gaspar. Doubtless, Werther was meant as something of a joke, for there is extant a letter to Macaulay telling of how Pearson and two friends, being impecunious, decided to write a book in 'the genuine gush style'.⁴¹

The work was completed in a fortnight and offered to Kegan Paul. But at the last minute the others (Raphael and Ethel of the book) left me in the lurch and the whole thing appears maimed and entirely thrown on me. Whatever you think of it (should you buy it, it is called the New Werther) you must on no account reveal to anyone my connection with it, as such revelation would be simply damping to my reputation, whatever that may be. So remember absolute secrecy.

We shall see that 'gush style' or not, this work, universally and rightly condemned by the critics, had a strong autobiographical content.⁴² Pearson's passion play was published anonymously and at his own expense. One doubts that it ever was performed, if only because it was over 200 pages in length. Once again the critics were unimpressed, though, as good Victorians, they clearly felt obliged to take seriously any play stuffed with the supposed speeches of Christ. A sample of this play is reproduced below. Mary Magdalen is the soliloquist.

Magd (taking up skull). Would that this tawdry gown of flesh
Were mingled with the dust like thing!
Where is that brain which thought and willed,
Which sinned and suffered? Where are those eyes
Which joy hath fired and grief had dulled?
The lips which kissed or muttered curse?
The tongue which careless trolled
A merry lay, or whisper'd words
Of dark and dreadful prophecy?—
Where are they all? Gone, gone for aye!
While memory forgets the name
That they once dreamt was their's and their's alone.

After the reception accorded to his literary efforts, Pearson, like so many before and after, appears to have accepted the judgement of his would-be peers, and wisely to have quit the field, enabling him to concentrate more upon philosophy, history and social criticism. The philosophical and historical essays, in fact, form the more distinguished part of Pearson's literary output, and show him as having talent in these directions. In 1880,⁴³ he wrote on 'Pollock's Spinoza'. In 1883, he wrote again on Spinoza,⁴⁴ and also on Kant⁴⁵ a philosopher whose words he had thoroughly assimilated by that time. There are also essays on 'Maimonides and Spinoza' and on various topics in German history.⁴⁶

Most notable, however, is the work on social criticism produced in the period before Pearson's appointment at University College. We soon see that if Pearson was not contributing frequently to the literature

of mathematics, it was because he was engaged in the more pressing task of putting the world to rights. We will have reason to return to this literature when we consider why Pearson was to found biometry and statistics. For the present it is surely sufficient to note that, as early on as 1881, Pearson was writing on⁴⁷ 'Anarchy'. In his paper, Pearson spoke of Marx and the history of the International, and moved on to divide socialists into two classes - into 'moderates' and 'extremists' in today's parlance, into social democrats and anarchists in Pearson's.

Pearson was grieved that at a meeting in London in 1881, 'a complete reunion must have taken place between the two parties'

As a result of this meeting Liebknicht and I believe ~~that~~ Bebel were imprisoned on their return to Germany, and we find Hasselmann and Fritzsche, (both colleagues of Liebknicht and Bebel) openly ~~expressing~~ approval in America of the assassination of the Czar! This approval is the death blow of German socialism (under its present form) as an open political party. We may then look upon the German socialists and the Russian nihilists for the future as the secret party of anarchy, and of this anarchy I propose to say a few words.

These few words were, in fact, many. But, the gist of what Pearson had to say was that there ~~was~~ a secret party of anarchy in Britain, disseminating its views in the pages of the Anarchist. Such documents probably had little effect on the better class of working man, but

in the dumb, helpless masses of our great towns, the Proletariat pure and simple, they foster the process of fermentation which is but too surely progressing. Truly did the manifesto of 1847 declare, that the Proletariat had nothing to lose, but a world to gain by a forcible revolution. This opinion is daily growing in the minds of the Proletariat themselves and the next ten years will certainly produce the fruit.

The chance visitor to London, said Pearson, who saw wealth and prosperity spread around 'would scarcely believe in the anarchical element existing in strength in such a town'. But were he to cross Blackfriars Bridge, or to go to the Borough or to Soho, his opinions as to the 'stability of society' would be shaken. He would see stunted forms and pallid faces - millions with nothing to lose, millions who

could 'sweep a few thousand police and soldiers before them as the wind blows a handful of chaff'. This lumpenproletariat, Pearson thought, were an entirely debased group of men and women. Even if a girl of their number was blessed with the 'divine gift of beauty', the only option for her was to cross to the West End, and to sell her body 'to the passions of the wealthy'. These people, said Pearson, 'are a fact, and the laws of political economy are not logical, barely historical, categories'. Again, he stressed, indicating a familiarity with communist literature, 'the proletariat have nothing to lose but their chains'. In short, there was a real prospect of bloody and ultimately useless revolution.

What was to be done about it? Philanthropy said Pearson, could do nothing, and was correctly bracketed by Carlyle with Italian organs and penny newspapers. Nothing could be expected from the government, for to deal with the problem of the lumpenproletariat, with what one historian has called 'Outcast London', would be to court electoral suicide. No, said Pearson, a revolution must be carried out from above. And, moreover, this would have to be done in a very particular way. Nothing could be expected from the Aristocracy of wealth, but some hierarchical ordering of society was needed if anarchy was to be avoided. Accordingly the question to be faced was the following one,

if we do not graduate society on the scale of wealth,
on what shall we graduate it?

His answer was that of a Cambridge third wrangler who felt himself undervalued in society,⁴⁸

So that while power material shall be divided as equally as may be between the various classes, power intellectual shall form a scale on which the necessary graduation of society may take place. Power intellectual shall determine whether the life-calling of a man is to scavenge the streets, or to guide the nation. With equal educational opportunities open to all, each would stand on his own footing. No man could live on the power material handed down to him by his ancestors, while power intellectual cannot be collected and bequeathed; it bears also no interest.

But, how was this new order to be brought about? What was wanted, said Pearson, was a new religion, to form a bond between class and class, and between man and man 'solely on the score of their manhood'. But, he

admitted, no such religion was in view. It was as if society was being led to the abyss by 'a Primeval force uncontrollable and not to be explained'. He had little to offer by way of detailed strategy.

I have discussed this paper in some detail as it shows that, as early on as 1881, Pearson saw himself as a socialist - but as a socialist of a very specific sort. He was an elitist socialist, a man who believed that the socialist route to progress lay not in some form of workers' control, but in the wise governance of an intellectual elite, who, having none of the pecuniary interests of the Capitalist, would be able to use their talents in directing society in the best manner. Pearson, as early on as 1881, was an outstanding example of this sort of socialist, and, unlike many others, did not mince his words. We shall see, again and again, that the idea of a rule by a socialist elite was a dominating passion. In fact, his later career as eugenicist may be interpreted as an attempt to provide the sort of guidance which such an elite would value

After the paper of 1881, the contributions to socialist literature began to flow rapidly. 1881 also saw articles entitled 'Songs of the proletariat' and 'Political economy for the proletariat'. 1883 saw Pearson's paper on 'The ethic of freethought', wherein he attempted to outline the ethical principles that should dominate in a rationally run society. 1883 was also the year of 'Songs of the socialists, done into English'. 1884 saw a paper on 'Socialism in theory and practice', which had been delivered to a 'working class audience'.⁴⁹ In these essays, Pearson argued that the 'laws' of political economy held only in certain historical phases of society, and were not the inexorable consequences of human nature. More importantly, he began the task of laying out in detail the principles by which socialist society should be run. His work on the ethic of freethought, for example, was an attempt to provide a new source of rational moral guidance, something to replace the Christianity which Pearson, qua freethinker, had abandoned. We will see the detailed development of his ideas in a later chapter, but it is worth noting that Pearson's ethic of freethought turns out to be one supposedly based in the objectivities of science. Science, in a sense, was Pearson's religion. No wonder, therefore, that some of his writings on the philosophy of science have the flavour of devotion literature. The most moral of men, Pearson argued 'must be in possession of the highest knowledge of his day'.⁵⁰

Pre-biometric London

1884 was something of a watershed for Pearson. At last he was able to escape from a law career to which he was ill adapted. After several unsuccessful attempts to obtain academic jobs he was appointed professor of applied mathematics and mechanics at University College London. The appointment came after Pearson had acted as a locum for Professor Rowe, professor of pure mathematics. Pearson had hoped to be made Rowe's successor, but the job went to M.J.M. Hill. But, in June 1884, he was appointed as successor to Henrici, the professor of applied mathematics. In the same year, Pearson was invited to complete Todhunter's History of elasticity⁵¹ and to complete and edit the late W.K. Clifford's Commonsense of the exact sciences.⁵²

1884 also brought recognition for Pearson's social writings, which, by then, included his 'Socialism in theory and practice' in which Pearson again laid out the case for an elitist and collectivist state and showed himself familiar with the works of Marx. Once again, he was emphatically anti-revolutionary in outlook, telling his audience that⁵³

You may accept it as a primary law of history, that no great change ever occurs with a leap; no great social reconstruction which will ever benefit any class of the community is ever brought about by a revolution. It is the result of a gradual growth, a progressive change, which we term an evolution. This is as much a law of history^{as} of nature.

Clearly, he seems to have accepted Darwin's evolutionary gradualism, i.e. the gradualism accepted by Weldon but rejected by Bateson, and to have been politically tied to it. In biology, as in politics, Pearson did not countenance the possibility of rapid, discontinuous change leading to stable and beneficial results. His form of elitist, gradualist socialism made him acceptable to established ^{middle class} radicals. In October 1884, he received an invitation from Caroline Wilson, the Hampstead disease famous for the shelter she extended to the Fabians, inviting him to participate in a select study group.⁵⁴

Enclosed with Mrs Wilson's letter of an invitation was a document entitled 'Society for the study and discussion of Le Capital'. People cited as members included F.Y. Edgeworth and Sidney Webb (economists), J. Hunter Watts and Dr Burns Gibson from the Democratic Federation, Edward Pease of the Fabians together with August Bordes,

Henri Bourden and Caroline Wilson, described as anarchists. Others listed as 'inquirers' included the Reverend L. Macdonald and Mrs Macdonald, Dr. W. Boulton, Ernest Hankin, Emma Brooke, Mademoiselle Bollinger and Arthur Wilson. Others, listed as 'members as yet doubtful' included Professor James Sully, H.H. Asquith, J.E. Pease, Mrs. Burns Gibson and L.K. Burton. Pearson was invited because it had been heard that he knew something of Marx. We know that he had read quite widely in Marx, especially from the paper on 'Anarchy' already referred to. It seems doubtful however that he became a member of the group as there are no records of his attending amongst his collected papers.⁵⁵

This may have been in part due to the circumstance that, Pearson, in 1884, was organising a group of his own - or, more precisely, a group that would discuss problems close to his freethinkers' heart. I am referring to the formation of a "men and womens' Club in 1884, whose purpose was to discuss, freely and frankly, all matters relating to relations between the sexes.

We can know a good deal about this group, as it kept good records. Particularly interesting is the autobiographical account kept by Maria Sharpe, the unmarried and younger sister of Pearson's friend Elizabeth Cobb, who was to be the secretary of the group and also later on to become Pearson's wife.⁵⁶ In her autobiographical account,⁵⁷ Maria Sharpe related that the idea of the club was first mooted to her by her sister Elizabeth Cobb in the autumn of 1884. Mrs Cobb passed on the suggestion that she (Maria) should join with her, Karl Pearson, Robert Parker and others 'in forming a club for discussing together subjects connected with the relations of the sexes'. Maria Sharpe was a member of a unitarian family and related, through another sister, to the famous Courtauld family. She had an open and liberated mind for a late-Victorian woman, and she recalled that she was attracted to the idea of such discussions because,⁵⁸

I had always since I had thought on such matters firmly believed that it was possible for men and woman to talk on them in perfect openness and perfect sincerity without harm, but that had been rather where there was practical work to be done. I had at one time talked a good deal to Sydney (Courtauld) on the subject of men and prostitution, but when I felt that he had given me all the definite knowledge in the matter which he was likely to give me I stopped the discussion.

Maria Sharpe recalled also that she was particularly interested in prostitution, which she described as 'the one great subject known to girls which fills them with an enquiring fearfulness when they first learn of it'. She believed it to be the 'one thought which lies at the back of any branch of the women's question'. She was generally favourable to the club because she saw it as a way of serving the 'women's cause, which, she said, she had had 'so much at heart since the earliest days I left school or before'. Accordingly, she agreed to join, though little more happened in 1884, except a meeting between her and her friend Lina Eckenstein and the authoress and feminist Olive Schreiner.⁵⁹ They met at Hastings.

The second period of Pearson's life, which we may take as that stretching from his appointment at University College to his meeting with Weldon, then, opens with his being ensconced among the London radical middle class, and with a good, if demanding job at University College.

He had given, and was to give, a great deal more of his time to radical writing than to applied mathematics, or, indeed, to any sort of mathematical research till the meeting with Weldon. During the period 1885 to 1890, the best of his radical writing was done; this was the period of the men and women's Club.

Returning to Maria Sharpe's account, we find that she and her sister were invited to tea with Pearson and his friend Ralph Thicknesse in March 1885. Thereafter, organisation of the club went at full tilt. The core group were joined by Kate Mills, Annie L. Eastty and others. The first meeting of the club was held on July 9 1885, at 27, Brunswick Gardens. Robert Parker was elected chairman, a set of rules was proposed and accepted. Pearson and Olive Schreiner were elected committee members, and Maria Sharpe secretary. Other members present were Elisabeth Cobb, Hume C. Pinsent, Isabella J. Clemes, Reginald J. Ryle, T.W. Rhys Davids, Constance Parker, Loetitia Sharpe, L. Agnes Jones and Annie L. Eastty.

The first paper read was Pearson's 'The woman's question'. This paper, later published in his collected of essays, the Ethic of freethought (1887), was remarkable for several reasons, not least of which was Pearson's use of Darwinian language in conducting his discussion of woman's place in society. He argued that there was a need for a 'science of sexualogy', and that 'it is the complete disregard of sexualogical difficulties which renders so superficial and unconvincing much of the women's rights talk', and ⁶⁰ went on to say tha

We have first to settle what is the physical capacity of woman, what would be the effect of her emancipation on her function of race-reproduction, before we can talk about her 'rights', which are, after all, only a vague description of what may be the fittest position for her, the sphere of her maximum usefulness in the developed society of the future.

The paper, as a whole, was discursive - taking in, for example, the possibility of a rational appraisal of the institution of marriage, and offering the possibility of a future state, 'when woman is truly educated and equally developed with man', in which marriage would be replaced by free union, along the lines pioneered by Lewis and Elliot and Wollstonecraft and Godwin. No doubt ^{Pearson} was thinking of his own parents when he asked whether 'marriage, lasting when the sympathy which led to it has died out, do ought but make two lives miserable'.⁶¹

Even in this early phase, we can see his moving slowly towards a propensity to sympathise with Galton in later years. We find him asking whether the progress of the 'great mass of the people', or, rather, the lack of it, might not be due to a correlation between being highly educated and lacking in philoprogenitiveness. Indeed, he was much interested in the question of the right to bear children.⁶²

Shall those who are diseased, shall those who are nighest to the brute, have the right to reproduce their like? Shall the reckless, the idle, be they poor or wealthy, those who follow mere instinct without reason, be the parents of future generations?...It is difficult to conceive any greater race crime. Out of the law of inherited characteristics spring problems which strike deeply into the very roots of our present social habits. It is not one, but a whole crop of questions which will be raised when the old idea of sex-relationship is once shaken.

Interestingly, the notion of race was coming to loom large in Pearson's mind. For example, in his remaining survey of the woman problem he spoke of the mob of women, the prostitutes, who paraded in London each night. These he said, constituted 'a great race-problem'. And, in his peroration, he looked forward to the possibility of a society in which women were emancipated but nevertheless submitted to 'the restraints demanded by social welfare, and to the conditions imposed by race permanence'.⁶³

The Men and Women's Club continued with regular meetings for the next four years. It offered to its members a staple fare of serious discussions about a variety of aspects of the relations between the sexes. It is hard to discern any deep coherence or thread in the minutes that remain, though it should be said that the matters discussed were ^{often} topical as, for example, when Robert Parker spoke on the Contagious diseases act, and on the views of Josephine Butler and others.

Similarly, a great deal of excitement and interest was created amongst club members by the revelations offered by the crusading W.T. Stead in the pages of the Pall Mall Gazette.⁶⁴ Most notable of these was his work on the 'Maiden tribute of modern Babylon' - an expose of the horrors of ~~club~~ ^{child} prostitution in the London of the 1880's. In general, however, the Club tended to organise talks and discussions about the nature of morality, the differences between men and women in respect of desires for sexual intercourse and the historical development of the relations between the sexes. Typical examples of the latter are Maria Sharpe's paper of 10 October 1887 on 'The laws and regulations dealing with prostitution in Western Europe from 800 AD to 1500' and Pearson's paper of 8 June 1886 on 'A sketch of sex relations in primitive and medieval Germany.'

The club survived for four years, until 1889. Its membership remained fairly constant, but it had a variety of guests of interest - these included, for example, the doctor H.B. Donkin, and persons considered for membership included Havelock Ellis and Eleanor Marx-Aveling.⁶⁵ The latter was turned down on account of her notoriety, for, above all things the members of the club desired to avoid notoriety and to escape the slightest breath of scandal. By 1889, it was dying, but managed to get together another three papers on heredity, of which the most notable was Pearson's talk on Galton's Natural inheritance, published in 1889.⁶⁶ To this talk, we shall return. For the moment, it should be noted that the club was disbanded in April 1889, mostly, one imagines, because it seems never to have had any very clear purposes in mind from the beginning, and because the members were getting heartily sick of hearing each other repeat the same opinions as to woman's place in society, real and ideal.

The period to 1890 then was one in which Pearson reinforced a reputation as radical thinker and writer. His output was stupendous. The bibliography of his works prepared by Morant indicates that, in the years 1885 to 1890 inclusive, he published no fewer than twenty one articles and books classifiable as 'literary and historical', including his Ethic of freethought of 1887 which was a compendium of his works.⁶⁷

During the same period, he published The common sense of the exact sciences and A history of the theory of elasticity and of the strength of materials from Galilei to the present time. On the literary and philosophical side of things, he was a model of productivity. This contrasted, with his productivity as a mathematician.⁶⁸ By 1890, he had published, in toto, only thirteen mathematical papers - that is to say, just over one a year since graduation. Moreover, his contributions to literature seem to have attracted a deal more attention than his mathematical efforts. We shall see that his commitment became exclusively mathematical only after he met with Weldon and began to produce biometrical papers - a circumstance which suggests that his biometrical papers absorbed not only his mathematical energies, but also those which had produced the vast flow of social and philosophical writing. This flow, interestingly, ceased almost on the instant when he began to take up biometry in the early 1890's.

Biometric London

The next obvious division of Pearson's life is that which stretches from 1890 to the death of Weldon in 1906. We have seen already how Weldon and Pearson came together in a context of university reform, and how, on Weldon's suggestion, Pearson wrote a paper, 'Contributions to the mathematical theory of evolution'. This, it transpires, was merely the first of a vast series, and, by 1906, we find Pearson at work on the fifteenth in a series of major memoirs, all entitled (with the exception of the first) 'Mathematical contributions to the theory of evolution'. In these papers and in associated ones, Pearson made massive contributions to biometry and thereby to statistics which, ^{as noted,} Churchill Eisenhart has depicted as having laid the foundations of the modern discipline of statistics. These contributions will be discussed in the next chapter, which addresses itself closely to the crucial historical question of why it was that Pearson, after meeting with Weldon, should have so totally devoted .

his energies to developing the new discipline of biometry, and to why biometry should have led to a series of major innovations in statistical theory, including^{as noted,} the method of moments for curve fitting, the series of Pearson frequency curves, the product-moment coefficient of correlation, the theory of multiple correlation and the chi-squared goodness of fit test. These are just a selection of his innovations, some of the ones which are frequently employed today in countless books and articles in natural and social science.

The 1890's were a crucial period for Pearson. They were years of full maturity when he was able to put his philosophical views before a large audience, to pursue his collaboration with Weldon and to make several very important human contacts.

I have mentioned already that, in 1885, Pearson edited and completed the late William Kingdom Clifford's Commonsense of the exact sciences. This was a work on the foundations of physics of a highly positivistic nature, a predecessor in form, to some of the works of the later 'Vienna Circle', formed originally as the Ernst Mach Verein. In the preface, Pearson recalled Clifford's dictum that 'no mathematician can give any meaning to the language about matter, force, inertia used in current text-books of mathematics', and cited 'the weighty authority of Professor Mach' as legitimating his own words on the laws of motion incorporated into the text. Pearson, already, like Clifford and Mach, was a prototypic 'logical positivist'.⁶⁹

These views became candidates for massive development and expansion, when, in 1890, Pearson applied for and received the post of Gresham lecturer in Geometry, which gave him an opportunity to talk to a popular audience on the topic of 'the scope and concepts of modern science'. The first lectures were given in March 1891, and, in the following year, their substance was published in the first edition of Pearson's famous manual of the philosophy of science The grammar of science.⁷¹ As might be expected, the lectures and the book sought to present an account of science that was free from the metaphysical entanglements commonly denounced by the followers of Mach, and, later on, of logical positivism. We shall return to Pearson's philosophy in the next chapter.

In November 1891 and in 1892, Pearson lectured on 'The geometry of statistics'. His lectures dealt with ways of representing statistical information graphically. They were followed, in late 1892, with a series on 'The laws of chance', described as being 'the elements of the theory of probability in its relation to thought and conduct.' In these lectures, Pearson began to incorporate some of Weldon's growing biometric data, and also sought to solve some of the standard philosophical problems of induction - though not very satisfactorily. He resigned the Gresham appointment in 1894.⁷²

But, brief though the appointment had been, it bore considerable fruit. It resulted in a great deal more than the Grammar of science, for there were also important human contacts made. One such was with George Udny Yule (1871-1951) who wrote to Pearson on March 6 1891 raising points about the Gresham lectures, which, presumably, Yule had attended. Yule's letter, and Pearson's reply are reproduced below. Yule had been studying with Hertz in Germany, and, accordingly, was well fitted to discuss Pearson's view of the nature of matter.⁷³

152 Gower St.,
W.C.
Friday (March 6th, 1891)

Dear Prof. Pearson,

I would like to ask you a few questions about your Gresham lectures, which have interested me very much. I suppose this letter will get to you sooner at Univ.Coll.

In your second lecture you said something to the effect that we find ourselves always imagining these ultimate particles of matter as bounded by geometrical surfaces'; if I understood right. What else can an atom or any thing else be bounded by?

In the abstract of Lecture II, at the end of paragraph (b) you say 'the ultimate element of matter, whether atom or not, we have no power to sensate. It must enable us to construct the mechanical universe, but cannot itself be mechanical? Why not? - or perhaps I should first ask - what do you mean by mechanical? Isn't a vortex ring or aether squirt or wrinkle 'mechanical', and are not these theoretical atom-forms what you mean by 'ultimate element of matter'?

At the end of yesterday's lecture you argued that the change of shape in the aether, necessary for it to transmit light waves etc., necessitates the existence of what one may call interatomic intensities. I don't quite see why it should. Couldn't there be waves in the sea even if water wasn't compressible? Again, as regards gravitation, wouldn't

the arms of a flywheel, for example, only hold it together the better if they had no atomic intensities.

Again, a year or so ago, when Dr. Fison read a paper before the college physical society on 'Modern views of electricity', someone or other who asked if this didn't merely reduce the action at a distance difficulty from small distances to smaller ones, was promptly met by the retort that 'aether was continuous' (from Dr. Fison). That was at least distinctly my impression.

I must apologise for this string of questions, and I am very glad to hear your lectures are going to be published, as one can't digest them offhand.

My main difficulty is certainly the statement about 'aether interstices', that 'aether as a medium exerting pressure leads us again to action at a distance between ultimate parts of aether. It seems to me it would exert pressure much better if it was continuous. You could shove a man down much better with a 'continuous' broomstick, than with an ordinary 'discontinuous' atomically constructed one.

Yours sincerely,

G.U. Yule.

Christchurch Cottage,
Hampstead. N.W.
April 20 '91.

Dear Mr. Yule,

I am very glad to think that the Gresham Lectures numbered such a one as yourself among the audience, and I will do my best to answer your questions.

In a lecture of my earlier course I pointed out that none of us had ever seen a geometrical surface. That no instrument could possibly construct one and that the atomic structure of matter conclusively showed us that when we spoke of a body as being bounded by a surface, we were speaking in the same metaphysical way as if we said that a swarm of bees was bounded by a surface. We have thus none of us any physical experience of a geometrical surface; it is thus an ideal we base upon certain of our sensations, a purely mental concept. Further I point out that the fact that an atom could vibrate showed it was a complex system, roughly speaking of an elastic and therefore not continuous nature. I spoke of the ultimate parts of an atom as prime atoms. I said we pictured these prime-atoms as bounded by geometrical surfaces. But if no physical body with which we have had experience is really rigid or bounded by a geometrical surface, - both these ideas being figments of the mind, we have no real right to suppose they exist for the atom. We simply can imagine the atom bounded by nothing else, but some people imagine bodies bounded by them and yet this is only an ideal boundary. Who then will venture to say that the metaphysical idea of a geometrical surface has a real existence for a physical, as apart from an ideal or symbolic atom?

I understand by mechanical a system of motion and moving things for which our ordinary ideas of force and mass, as summed up in the laws of motion hold. I do not think we have any right to assume those laws to hold for the ether-element, at any rate as at present stated. They probably, like the law of gravitation, largely flow from the constitution of heavy matter out of etherial matter. Granted that heavy matter is ether in spin or some form of motion, the mass of heavy matter must involve the velocity of this motion as a factor, for if it ceases to be, if we stop the motion. Hence the mass of heavy matter is of a totally different type to the mass of an ether element. And the action of ether element on heavy particle may obey something different from the 3rd law (of ratio of spurts = inverse ratio of masses, for the ratio of the masses is no longer a mere number - i.e. is probably the square of a velocity). Further the 2nd law of motion is probably untrue for molecules as well as for ether-elements. Thus the further we go back, the more we may find the laws of motion, mechanism, to be due to the structure of matter (just as the law of gravitation is due to ether's structure). A Vortex ring is certainly mechanical, but it won't work. An ether-squirt and a wrinkle are not purely mechanical, (the latter especially not - it is the first stage to explaining by geometry) for they very possibly involve the meta-physical notions of a fourth dimension. From the ultimate element whatever it may be all the laws of motion, mechanism, must flow, but this means that it cannot itself be mechanical in our ordinary sense. It cannot at the same time explain the laws of motion and obey them. Now as to the ether it transmits with finite velocity some form of motion, it is therefore capable of strain, suppose this indeed to be only shearing strain, still a shear may be resolved into a stretch and a squeeze and these notions again involve the idea of bringing some parts together and separating others. You will find this idea perfectly inconceivable with a medium which is perfectly continuous. Try and conceive it if the ether-elements were made of rigid closely packed elements! Try and conceive motion in an incompressible jelly fixed to the surface say of a sphere! - there could be no waves in the sea if you put a rigid plane on the top of it, or extended it to infinity and supposed it absolutely incompressible. Waves denote change in shape and change in shape must denote room to change shape in, even if the change of shape denoted no change of volume. The problem of action at a distance is just as real I fancy for the elements of ether as for elements of heavy matter, only if we explain the latter by the former we have got at least one less problem to solve.

As to your broomstick, if it were of 'continuous', i.e. absolutely inelastic material no wave could go above it, and I should think it would have instantaneously an infinitely great force applied to an infinitely small part, eg. a piece would snap off it. But the idea of absolute rigidity is purely ideal, it is not a limit to the actual, for in all cases we are easily able to show that the supposed rigid body is really strained. I cannot conceive in fact stress without strain, but of this I am at any rate quite certain that without strain no vibrations could be propagated with finite velocity, which is certainly a feature of the ether. There is the kernel of the matter, the ether possesses

some of the properties of an elastic solid - these seem to be inconceivable in the case of an absolutely continuous and therefore rigid body.

Yours very sincerely
Karl Pearson.

The letter and response must have led to mutual sympathy, for, in June 1893, Pearson ^{was} ^{to} ^{be} found writing to Yule offering him work as a teaching assistant. Yule accepted readily, thereby entering into Pearson's life, and, as we shall see, into the history of statistics.

Another contact made was with Galton. Pearson wrote to him on 28 February 1893, asking him if he could deputize at a fee of seven guineas, possibly with a lecture on the use of the laws of chance in anthropology.⁷⁴ Weldon had been booked for a similar lecture, but with a biological emphasis.

Earlier on, in 1892, as a consequent of his writings on University reform, Pearson had made contact with Alice Lee, then working as a lecturer at Bedford College London. Pearson wrote back (February 19, 1892) reminding Lee that what he wanted was 'a great teaching University in London on the scale of Vienna or Berlin', and deprecating the scheme for an Albert University, then still in the air. Out of this introduction came another of Pearson's longest partnerships. Over the next several years, Alice Lee joined him as a biometric helper, and is perhaps most famous for her joint paper with Pearson on the laws of inheritance in man, published in 1903.⁷⁵

So far then, we have followed Pearson as far as 1893. By then he was married and had a family. His Grammar of science was, on the whole, well received, frequently being lauded for its Kantian overtone. The Speaker for example, wrote as follows⁷⁶

Now if Professor Pearson treats his analysis - as we think he does - as covering all mental phenomena, he does what Kant would have done had he stopped short at the end of his Transcendental Analytic.

Again, we shall return to this Kantian influence.

By the end of 1893, Pearson had almost but not quite entirely abandoned 'literary and historical' writings, and was beginning to focus all of his

energies on the development of biometry. Thus, for example, his paper ⁷⁷ of 1896, 'Contributions to the theory of evolution. III. Regression, heredity and Panmixia' advanced Galton's statistical methods of dealing with heredity by introducing the product-moment coefficient of correlation and by developing ideas of multiple correlation. Thus it was that statistical methods and biological inquiry so frequently went hand in hand in the pursuit of biometry. The consequences for theoretical statistics of this enterprise have been noted in the passages from Stouffer and Eisenhart cited above.

The statistical ideas which Pearson was developing were, of course, transmissible, and it is not surprising that, in 1894, Pearson began to lecture on the new statistical methods, to the delight of Yule, Alice Lee and other auditors.⁷⁸

The 1890's were marked not only by steady progress in biometric work and in the development of statistical teaching at University College, but also by a growing friendship with Galton with whom Pearson had further dealings during the life of Galton's and Weldon's 'Evolution committee'. Pearson acted as a mathematical consultant, but, as we shall see, resigned his membership in 1897 after ^{he} and Weldon had become exasperated by the growing influence of Bateson on the Committee.

But, resignation or no, Pearson was to receive the Royal Society's Darwin medal in the following year. ~~He wrote~~ to Galton, who, one supposes, had assisted in obtaining the reward for Pearson, that,⁷⁹

It seems to me that the only way I can look upon it, is as a recognition of method. Therein, I think, must be the satisfaction to yourself - any mathematician could have done what I have done, a dozen of the better men far better, - especially if they had had the suggestive Weldon almost daily at lunch for four or five years. Hence the medal is not a recognition of my mathematics, but of the fact that the quantitative treatment of biological problems, which you have initiated is to be in future one of the great instruments of research in matters of evolution. It is a recognition which justified your methods, and will throw new life into all the little groups, who have so unselfishly assisted in most laborious arithmetic during the past few years.

Hereafter, the ties with Galton were considerably strengthened. Pearson dedicated pieces of his biometric work to Galton, and enjoyed a full correspondence with him, with Pearson always expressing genuine respect for Galton and his work. A most significant letter, indicating a growing harmony of interest between Pearson and Galton passed from the latter to the former in the first month of 1901. Pearson expressed a great regard for Galton's ideas^{for} embarking upon active eugenic propagandising - something which, as noted in the previous chapter, Galton had fought shy of before 1900.⁸⁰

7 Well Road,
Hampstead N.W.
Jan 10, 1901.

My dear Mr Galton,

It would be a very great pleasure to me to know you were going to take the field with regard to what I am convinced is of the greatest national importance, - the breeding from the fitter stocks. If one could only get some one to awaken the nation with regard to its future! The statesmen, who really have the ear of the populace, never think of the future. They will not touch the question of coal supply nor that of fertility, and yet I am convinced these are far more important for the very existence of the nation than any question of local government, church discipline, or even technical education! I think I told you we had nearly completed the reduction of our measurement on 1100 families and one after another of the results confirm the higher series of values, about .5 for parental correlation, that I found from the eye and horse colour data. I shall probably not publish these results for some time, as I have half made up my mind to accept an invitation to lecture at the Lowell Institute in Boston this year and these materials would be a good basis for lectures on Heredity. But they emphasise even more emphatically than your earlier value of $1/3$, the opinions you have expressed on the great part played by good stock in the community. Heredity is really more intense than we supposed it to be 10 years ago. Cannot this be brought forcibly home to our rulers and social reformers?

Now the difficulty in this case seems to me to be twofold. How can you (1) stop the fertility of the poor stock and (11) multiply that of the good? The middle classes are I take it the result of a pretty long process of selection in this country, and I believe that they alone are the classes who largely ensure. Your scheme therefore would at first apply only to them, and indeed to the best of them, for the others would not care a rap for a good bill of health, more than they do for any moral suasion. You might influence by your health degree a small percentage of the whole community, say 4 percent, but this percentage is probably identical with those you could equally well influence by moral suasion. I mean by preaching the gospel that the stability of the nation depends upon the good stocks breeding fully and the weak exhibiting restraint.

But now are you going to get the better ^{class} ~~chap~~ workman to see that his checking the size of his family may make matters easier for him, but is at the expense of the nation's future? His is really unreachable by an assurance scheme, unless you could attach your health degree to the proposals for old age pensions. That seems to me a point worth thinking about. As I have said elsewhere it seems to me that only socialistic measures can touch this population question.

Even if you can by moral suasion lead the better ^{class} ~~chaps~~ artisans and the middle classes to see that limitation of the family may be anti-social (and I believe it might be possible), how are you going to check the unlimited production of the worse stocks? The 'Neomalthusians' as I know from sad experience - abuses any one who like myself ventures to criticise their doctrine of limitation, unless it be accompanied by the words "of the poor stocks first" but this abuse is nothing to what one will arouse, if one ventures to assert that the huge charities providing for the children of the incapable are a national curse and not a blessing; that the "widow with seven children all dependant upon her; husband a clerk who died of consumption aged 35", and who seeks your aid to get her children into Reedham, is really a moral criminal and not an object for pity. How can a health degree affect this source of rottenness? I fear hardly at all. Your only hope is to impress upon the few who really lead the nation, that the matter is one for legislation, that although we have got rid of Gilbert's Act, the workhouse and charity systems can still be sapping our national vigour, when coupled with a widespread neomalthusianism due in the main to Bradlaugh - among the better working classes.

What then it seems to me that we need most of at the present time, is some work in season, something that will bring home to thinking men the urgency of the fertility question in this country. There is no man, who would be listened to in this matter in the same way as yourself. You are known as one who set the whole scientific treatment of heredity going; no one has ever suspected you of being in the least a 'crank', or having 'views' to air. You will be listened to and it will be recognised that you write out of a spirit of pure patriotism. There is no one else. I believe of whom this could be said, certainly no one who would be listened to in the same way. Let us have (a) known facts of heredity (b) influence of relative fertility on national vigour (c) actual statistics of birth rates of different stocks, and (d) proposed remedies (only, if the health degree, tack it onto old age pensions) brought home to those who think for the nation.

Always sincerely yours
K. Pearson.

So, by the very early years of the twentieth century,⁸¹ *it would appear that* Pearson was a totally convinced eugenicist, and, as we shall see in the chapter devoted to eugenics, he was *beginning to write about* eugenics in a most forceful tone, frequently ascribing what he saw as an increasing decline in Britain's economic and political standing in the world to a decline in the birth-rate among so-called 'good stocks' in society.

At the same time, Pearson and Weldon were building up the infant Biometrika, with financial assistance from Galton, and were spending large amounts of energy in combating what they perceived as the menace of Mendelian genetics, which was being developed in Cambridge by Weldon's former colleague William Bateson. The conflict, this so-called 'biometric-Mendelian' debate, dealt with in a later chapter, occupied much of the last six years of Weldon's life, and it may be thought that his sudden demise in 1906 was hastened by the over-work that he indulged in during the conflict.

Pearson, of course, could not devote all of his energies to combating Mendelism - though, as we shall see, he gave the Mendelians the rough edge of his tongue on several occasions and remained implacably hostile to the emerging science of genetics. He was busy in other directions, developing his statistics teaching for example - which, until 1915 remained an exclusively post-graduate exercise, developing the ideas of many persons subsequently to achieve scientific distinction, or high posts, or both. Things were no doubt assisted by a grant from the Drapers' Company which was made to Pearson's department, starting in 1902, and continued annually until 1932, at a rate of £500 per annum.⁸² From quite an early stage, Pearson referred to the section of his applied mathematics department responsible for biometric work as the 'Biometric Laboratory'.

The early twentieth century was also the time of the beginning of Pearson's involvement in institutionalized eugenics. As noted, Galton endowed a 'Eugenics record office' in 1904, by presenting £1500 to the University of London, which provided rooms in Gower Street. Edgar Schuster was appointed as first research fellow and Ethel Elderton, sister of the actuary W. Palin Elderton, was made his assistant.

From the earliest date, Pearson was involved in the setting up of this enterprise. He was a member of the small committee which processed and modified Galton's ideas in the week following upon Galton's visit to put the plan to his friend, Sir Arthur Rucker, principal of the University of London. Two letters from Pearson to Galton show their closeness. In the first, Pearson informs Galton of his place on the committee. In the second, he supports Schuster as a candidate for the fellowship and argues against giving the job to A.D. Darbishire, who, as we shall see, worked as Weldon's assistant whilst Weldon was struggling against the Mendelians, but let the side down by converting to Mendelism.⁸³

7 Well Road,
Hampstead N.W.
October 11, 1904.

My dear Francis Galton,

I have just been asked by the Registrar of the University to form one of a committee consisting of Sir Edward Busk and Mr Mackinder to discuss with you the form to be taken by your offer to establish a "Research Fellowship in Eugenics". Of course I shall be only too glad to help if I can in the matter, but I write to tell you, as I am not certain that you would expect to see me on the committee. I need hardly say that I think the suggestion a good one. I think immense good can be done by a careful statistical study of what tends to deteriorate and to strengthen a nation physically and mentally from the standpoint of the individuals from which it is reproduced. I hope in this sense that Eugenics covers not only a theory of better breeding, but a study also of Kakogenics or of the bad breedings current at present.

With kind regards to Miss Biggs - may I know when the portrait will be ready to be carried off. - I am

always yours sincerely,
Karl Pearson.

7 Well Road,
Hampstead N.W.
Dec.20th 1904

My dear Francis Galton,

I am very glad that you are more satisfied with the three men who are now on the list for the Eugenics Fellowship. Schuster and Darbishire are the two I know anything about, and I would like to say quite frankly to you what I think and know about them.

Schuster in many ways would be an admirable candidate. He has manners, wealth, and some experience.....

On the other hand I do not think Darbishire would in the end do the new appointment credit. It is perfectly true that he has good superficial manners and appearance but I do not think he has any real grit, and I should frankly be sorry if you selected him.....

Always yours affectionately,
Karl Pearson

In fact, Rucker realised something of the power of Pearson's personality, and wrote to Galton recommending that the research fellow should be kept separate from Pearson's bailiwick.⁸⁴ The consequence of doing otherwise would be that there would be a quarrel, or Schuster would be dominated by Pearson.

Nonetheless, Pearson does seem to have exerted influence, and not to have been short of ideas for action when Galton was troubled. The letter reproduced in part below shows this. Schuster was contemplating a move, and we see that Pearson was already talking with Galton about a 'Galton professorship of eugenics'.⁸⁵

7, Well Road,
Hampstead N.W.
October 27, 1906

My dear Francis Galton

This is only an interim letter, because I must really find out from Schuster what his actual wishes are. My impression - but it may be erroneous, is that he is rather restive at doing no biological work. He wanted to make experiments with mice bearing on the influence of size of families and nutrition.....

Your foundation would be a splendid thing, and, I think, there is a great future before eugenics. But my advice would be don't hamper it. You are going to be with us a long time yet, and in those years many young men will or may come forward. Hooker is no

doubt an able man, but he is far from a trained statistician; he is not in the same class with men like Palin Elderton, Macdonnell or Yule. If you got him as a fellow now subject to a post obit professorship, there would be little incitement to exertion and you would feel much hampered, if he did nothing of mark during your lifetime. It would be safer by far to test a number of younger men, without any restrictions as to the future and out of the group one might rise to great things and be worthy of the Galton professorship of eugenics!

.....

Always your affectionately

Karl Pearson.

Eugenic London

1906 was the year of Weldon's death. But, it was also the year in which Galton handed over control of the Eugenics Record Office to Pearson, enabling him to run it in conjunction with his Biometric Laboratory. David Heron replaced Schuster, who resigned, and the Office was transferred into University College, where it became known as the Galton Eugenics Laboratory. As E.S. Pearson has noted, Karl Pearson was now a very busy man⁸⁶

And so at the beginning of 1907 we find Pearson head of the Department of applied mathematics, in charge of the drawing office for engineering students, giving evening classes in astronomy, the director of two research laboratories and the editor of their various series of publications and of Biometrika.

This state of affairs continued for just another four years, with the two laboratories producing a spate of research memoirs in eugenics and biometrics. In 1911, Galton died and left £45,000 ^{to the University of London} for a Galton professorship of eugenics, to be offered to Pearson in the first instance. Pearson accepted gladly, and, aged 54, left the department of applied mathematics which he had overseen for 27 years. By joining the Biometric and Galton laboratories, both of which had acquired funding (from the Drapers' Company and the Galton bequest), Pearson was able to found a new Department of applied statistics around his new chair. It was the first such department in the anglophone world, and, for long, served as the premier source of statistical tuition.⁸⁷

Development was held up by the first war, which saw Pearson and his assistants working on ordnance problems. But, by the end of the war, Pearson was to be found in charge of a thriving department of applied statistics, in which eugenic, statistical and biometric work were carried out by Pearson's assistants and research students. An honours

degree in statistics was introduced in 1915, thereby opening up statistics as a subject for undergraduate education. In 1920, new buildings were opened by the minister of health. It was in these that E.S. Pearson and Jerzy Neyman were to collaborate in their own crucial contributions to statistics, that Pearson, in 1926, was to found a new journal, Annals of eugenics - now known as the Annals of human genetics - and that R.A. Fisher was to take over the Galton chair on Pearson's retirement.

Mention of Fisher is just one reminder that this overview of Pearson's career has been extremely selective and patchy - for his interaction with Fisher was of the greatest interest and importance. Nevertheless, the overview has depicted some of the outstanding features of the career of the man who did most to develop some of the notions due to Galton, notably, of course, the ideas of a statistical methodology for use in the biological and human sciences, and of an eugenic salvation for British society.

We have yet to see the full extent of these developments - Pearson's statistical ideas, for example, have been reverently mentioned rather than described. The details of these developments and their rationales form the subjects of subsequent chapters. So far I have done little more than to sketch out some of the bones of the story of Pearson and his biometric school, and it remains to trace the influence of these on the biological and social sciences, and, indeed, to explain all of these things. Before getting down to this, it is necessary to discuss one further 'first generation' Galtonian, and to mention some of the 'second generation' Galtonians of consequence in the history of statistics.

F.Y. Edgeworth (1845-1926)

There is only a thin sense in which Francis Ysidro Edgeworth may be described as a Galtonian, but he certainly deserves a mention.⁸⁸ The fifth son of a sixth son, he eventually succeeded to the family estate of Edgeworthstown County Longford, in 1911. The family had been established there by Queen Elizabeth the first. Edgeworth took a first in classics at Balliol College Oxford. Thereafter, like Pearson, he made moves in the direction of a legal career, and was called to the bar in 1897. Apparently,

the law did not engage all of his attention, and, after a spell as a lecturer in logic at King's College London, he was, in 1891, appointed to the Drummond chair of political economy at Oxford. He served as president of the economic section of the British Association in 1889 and 1922, acted as president of the Royal Statistical Society and was first editor of the Economic journal.

His major early works were his New and old methods of ethics (1877) and his Mathematical psychics 1881. The details of these need not concern us, but it is worth noting that the Mathematical psychics had two parts, 'concerned respectively with principles and practice, root and fruit, the applicability and the application of mathematics to sociology.' Edgeworth also pondered some of the philosophical problems of probability theory, and is particularly famous for his essay on the 'philosophy of chance', published in Mind for 1884.⁸⁹

Edgeworth and Galton, therefore, had a certain amount in common. Both had an interest in the theory of chance, and both were anxious to apply mathematical methods to human phenomena. Edgeworth reviewed Galton's Natural inheritance in Nature, and spoke highly of Galton's new statistical approach.⁹⁰

One doubts that they had the basis for a close intellectual relationship, as there is little to suggest that Edgeworth's thought had any eugenic tinge. Certainly, the two men must have been separated, both physically and intellectually by Edgeworth's appointment to the Oxford chair in 1891. But, this notwithstanding, Edgeworth did do some work on Galtonian ideas in his long and distinguished career, notably on the theory of correlation. In 1892, for example, he showed how Galton's account of the correlation between two variables could be extended to three, four and more variables. Certainly, by the time of his death, his contributions to various areas of statistics were sufficient to lead Bowley to write a monograph on the topic.⁹¹

The 'Biometric school'.

I have passed rapidly over the work of Edgeworth because, though he was a most distinguished man, he was not a conscious contributor to the Galtonian tradition, which matched statistical methods to social biology.

Instead, I wish to pass on to an account of an important aspect of the influence of Karl Pearson, the leading 'first generation' Galtonian. For, it was his glory to have created a strong school of statisticians, and it is in the creation of this school that his chief claim to historical influence must reside. As Helen Walker has noted, his developments of statistical technique were tremendous, but he is better perceived as a man who moved the scientific world from a state of disinterest in statistical studies 'to a situation in which a large number of well trained persons were eagerly at work developing new theory, gathering and analyzing statistical data from every content field, computing tables, and re-examining the foundations of statistical philosophy.' As Walker notes ⁹²

His concept of a general methodology underlying all science is one of the great contributions to the world. His laboratory was a world centre in which men from all countries studied and from which they returned to set fires in their own homelands.

In what follows, I can hardly attempt much more than to give a list of names and achievements, mostly with a view to suggesting the sheer influence and importance of Pearson, whose work will be explained and analysed in the next chapter. But, even in the course of a brief chronology, we will be able to see a distinct 'generation effect'. Pearson was a Galtonian and a biometrician - frequently, his followers had little interest in social biology, and approximated far more closely to the more modern role of 'statistician' than to the older role of 'biometrician'.

G.U. Yule. (1871-1951)

Pearson's first major follower was G.U. Yule,⁹³ son of Sir George Udny Yule (1813-1886), a noted Indian civil servant. Yule studied engineering at University College, and, as we have seen, met up with Pearson. From 1890 to 1892 he worked as an engineer, and thereafter worked with Hertz until joining Pearson's applied mathematics department. He remained until 1899, when ^{for financial reasons} he took an administrative job. In 1912 he was appointed lecturer in statistics at Cambridge, whence he supervised the appearance of many editions of his standard text, the Introduction to theory of statistics. By 1950, this was into its fourteenth edition and had done 39 years of service in introducing many generations of students to the statistical ideas which Yule had first encountered in 1894 when

he and Alice Lee attended Pearson's first course of statistical lectures. Yule, unlike Pearson, was an active member of the Royal Statistical Society and produced a considerable amount of original statistical work. He is especially famous for his work on measures of association other than the correlation coefficient. Yule's defence of these measures, conjoined with criticism of Pearson's uses of the correlation coefficient, led to a long long conflict with Pearson after 1905.⁹⁴ For the present, we may ignore this conflict. It is sufficient to note that ^{that} most influential figure Yule began his work in statistics as a consequence of working with Pearson.

W.S. Gossett (1876-1937)

Yule turned to statistics as a consequence of meeting Pearson. The same cannot be said of W.S. Gossett, 'Student', for he contacted Pearson with the hope of obtaining assistance with statistical problems which he had encountered in his employment as a scientist in the brewery firm of Messrs Arthur Guinness Son and Co. By 1904, he alerted his employers to the problems associated with interpreting sample data, pointing out that⁹⁵

'Results are only valuable when the amount by which they probably differ from the truth is so small as to be insignificant for the purposes of the experiment.'

In 1905, in order to pursue the matter, Gossett met with Pearson, and, as a consequence, became a part-time worker in Pearson's biometric laboratory. The nature of Gossett's practical problem-situation led him to investigate the behaviour of frequency constants in small samples. Thus it was that 'Student' came to produce his major contributions to small-sample statistics.⁹⁶

Major Greenwood (1880-1949)

The great medical statistician Major Greenwood was another product of Pearson's biometric laboratory. He published his first paper in Biometrika in 1904. At one time, he confessed, he developed 'an almost schoolgirl passion' for Pearson. He was appointed to the appointment of statistician in the Lister Institute in 1910, the first such post of its kind in Britain. And, Hogben claims,⁹⁷ the uptake of statistical methods into medicine was due in no small measure to Greenwood's pioneer work on large scale trials to assess the efficacy of prophylactic and therapeutic measures. In 1927, Greenwood became first professor of epidemiology and medical statistics

at the London School of Hygiene. In the early twentieth century, he appears to have combined an admiration for Pearson's statistics with a sympathy for Pearson's hereditarian social biology. Later on, this regard for hereditarian ideas diminished, but his regard for Pearson's statistical ideas which he had first learned in the biometric laboratory seems never to have dimmed.

R.A. Fisher (1890-1962)

It would be wrong to overestimate the influence of Pearson upon Fisher - though, it would also be easy to underestimate it. Fisher was never a student in Pearson's biometric laboratory, but, we know, his entrée into biometrics and statistics came via his reading of Pearson's work while still a mathematics undergraduate at Cambridge. We shall see more of the impact of Pearson's brand of statistics and social biology upon Fisher in later chapters, for the moment it is worth noting that it was very considerable.

Other biometric workers

I have made considerable play of Pearson's influence on major statisticians such as Gossett and Yule because I am most anxious to establish his historical significance within the development of statistics. In the chapters which follow, it will be seen that this influence was by no means confined to people whom we generally categorise as 'statisticians'. We shall see that Pearson's promoted statistics as a universal methodology for the social sciences, and with considerable effect. For the moment, however, it is perhaps a good idea to recall briefly some of the other workers who trained or worked in Pearson's laboratories. The list is compiled mainly from a report made to the Drapers Company in 1918.⁹⁸ Workers mentioned therein include Alice Lee, W.R. MacDonnell (biometrician), Ernest Warren (zoologist), Raymond Pearl (American biologist and biometrician of great note), J. Arthur Harris (American botanist), W.F. Harvey (Director of the Pasteur Institute of India), J.F. Tocher (biometrician), Gustav Jaederholm (psychologist and pioneer student of mental deficiency), Charles Goring (Home Office financed investigator of the British convict), William Brown (professor of psychology at King's College London), E.C. Snow and L. Isserlis (mathematicians), A.M. Carr-Saunders (future director of the London School of Economics), H.J. Laski (famous labour politician) and

L.N.G. Filon (mathematician). Others associated with Pearson's laboratories included Julia Bell (medical researcher), Ethel Elderton and David Heron (eugenists), W. Palin Elderton (actuary), S. Stouffer (sociologist) and, of course - looking to a later period, Jerzy Neyman.

These people - in differing degrees - constituted Pearson's 'biometric school'. Were we to trace their influence in the development and application of statistical methods, we should, assuredly, come to agree with Helen Walker's view of Pearson, which was namely that⁹⁹

Few men in all the history of science have stimulated so many other people to cultivate and to enlarge the fields they had planted.

- 1 For an overview, see K.Pearson, 'W.F.R.Weldon, 1860 -1906', Biometrika, 5 (1906), 1 - 52.
- 2 Information taken from J.G.Crowther, British scientists of the twentieth century, London (1952), 280.
- 3 For details of Newton's life and career, see A.F.R.Wollaston, Life of Alfred Newton, London (1921)
- 4 For information on Babington, see Memoirs, journal and botanical correspondence of C.C.Babington, Cambridge (1897).
- 5 Thanks are offered to J.R.Baker for permission to reproduce this correspondence, dated 28 September, 1948.
- 6 For details of Huxley, see Cyril Bibby, T.H.Huxley; scientist, humanist and educator, London (1959).
- 7 Details in William Coleman, Biology in the nineteenth century, London (1971). See also, Garland Allen, Life science in the twentieth century, London (1975).
- 8 See L.Hogben, 'The origins of the society', in M.A. Sleight, The origins and history of the Society for Experimental Biology, London (1966)
- 9 For details of Lankester, see the Dictionary of National Biography
- 10 See, e.g., the University College London Calendar for 1890
- 11 Galton Archive, University College London.
- 12 W.F.R.Weldon, 'On certain correlated variations in Carcinus moenas', Proc.Roy.Soc., 54 (1893), 329
- 13 This is discussed in Bibby, op.cit. (footnote 6), 222
- 14 Cited by K.Pearson in op.cit. (footnote 1), 20. See also, K.Pearson The new University for London. A guide to its history and achievement of its objects, London (1912)
- 15 Pearson's letter appeared in the Times for 3 December 1892. Huxley's appeared in the Times for 6 December 1892
- 16 Pearson op.cit. (note 1) 21. Weldon's respect for Huxley can be gathered from the account of Huxley which he wrote for the Dictionary of National Biography.
- 17 Pearson Archive, University College London
- 18 K.Pearson, 'Contributions to the mathematical theory of evolution', Phil.Trans.Roy.Soc., 185A (1894), 71 - 110.
- 19 C.Darwin, The Origin of Species: a variorum text, ed. Morse Peckham, Philadelphia (1959), 267, sentence 382.65.0.50.369.
- 20 T.H.Huxley, 'The origin of species', in T.H.Huxley, Collected Essays, ^{vol 2,} London (1899). See p.77 'Mr. Darwin's position might, we think, have been even stronger than it is if he had not embarrassed himself with the aphorism "Natura non facit saltum".'

- 21 See, for an account, B.Bateson, William Bateson F.R.S., Cambridge (1929). See also my chapter 5.
- 22 For an account, see K.Pearson, op.cit. (footnote 1), 23.
- 23 W.F.R.Weldon, 'An attempt to measure the death rate due to the selective destruction of Carcinus moenas with respect to a particular dimension', Proc.Roy.Soc., 57 (1895), 260 - 379.
- 24 W.F.R.Weldon, 'Remarks on variation in animals and plants', Proc. Roy.Soc. , 57 (1895), 379 - 382.
- 25 For an account of the formation of Biometrika, see K.Pearson, op.cit. (footnote 1), 35.
- 26 Extant literature on Pearson includes the following items. Obituary notice by G.U.Yule, in Obituary notices of Fellows of the Royal Society of London ,2,(1936),73 - 104. 'S.S.Wilks,'Karl Pearson, founder of the science of statistics', Scientific monthly , 53, (1941),249 - 253. Helen Walker, 'The contributions of Karl Pearson', Jnl.Am.Stat.Assoc., 53 (1958),11 - 22. See also, Bernard Sempel, 'Karl Pearson, socialist and Darwinist', Brit. jnl. sociology ,9, (1958),111 - 125. S.A.Stouffer, 'Karl Pearson.An appreciation on the 100th. anniversary of his birth', Jnl. Am.Stat. Assoc., 53 (1958),23 - 27. E.S.Pearson, Karl Pearson:An appreciation of some aspects of his life and work , Cambridge (1938).
- 27 J.B.S.Haldane, Karl Pearson, 1857 - 1957 , privately printed by the Biometrika trustees, London (1958)
- 28 Information contained in a Pearson family album, kindly loaned by E.S.Pearson.
- 29 Pearson Archive, University College London.
- 30 Ibid.
- 31 For details of King's and other Cambridge colleges of the period, see Sheldon Rothblatt, The revolution of the dons, London (1968). For details of the Etonian connection, see pp.223 - 227. Rothblatt discusses the careers of Browning and Bradshaw.
- 32 For details, see E.S.Pearson, op.cit. (note 26), 4.
- 33 For details of Robert John, Baron Parker of Waddington, 1857 -1918, see the Dictionary of National Biography
- 34 See Churchill Eisenhart's article on Pearson the the Dictionary of Scientific Biography ,vol.10 (New York (1974),447 - 473.
- 35 Stouffer, op.cit. (note 26), 23.
- 36 Op.cit. (note 26)
- 37 Pearson Archive, University College London.

- 38 Ibid
- 39 Ibid
- 40 Ibid
- 41 Ibid
- 42 The central character in Werther bears an uncanny resemblance to Pearson - an Englishman studying in Germany, coming to grips with physics, socialism and the philosophy of Kant. In the Werther, for example, there is a clear reference to an Alpine trip with Oscar Browning in 1875. Browning is referred to as 'the prophet of a sham culture, a paper knife in human form'. See pp 89 -90. Pearson's passion play was published, in 1882 by E. Johnson, at Cambridge. The citation below is from page 131.
- 43 K. Pearson, in The Cambridge review, 2 (1880), 94 - 96.
- 44 K. Pearson, 'Maimonides and Spinoza', Mind, 8 (1883) 338 -353.
- 45 K. Pearson, 'Kuno Fischer's new critique of Kant', Cambridge review, 5 (1883), 109 - 111.
- 46 See e.g., K. Pearson 'The kingdom of God in Munster', The modern review, 5 (1884), 29 - 56 and 259 - 283.
- 47 K. Pearson, 'Anarchy' ,Cambridge review, 2 (1881), 268 - 270.
- 48 Ibid, 270
- 49 K. Pearson, Socialism in theory and practice, William Reeves, London (1884)
- 50 K. Pearson, The ethic of freethought, London (1888), 18.
- 51 ed. K. Pearson, A history of the theory of elasticity and of the strength of materials from Galilei to the present time , Cambridge (1884)
- 52 edited and completed by Karl Pearson The common sense of the exact sciences, London (1885).
- 53 This essay was republished in Pearson's book, The ethic of freethought, London (1888). See p. 363 for the quotation.
- 54 See C. Wilson to Karl Pearson, 22 October 1884, held in the Pearson archive, University College London. For details of Mrs Wilson, see E.R. Pease, History of the Fabian Society, 3rd. edn, London (1963), 49.
- 55 Letters in the Pearson archive, show that Pearson had a continuing relation with George Bernard Shaw. Shaw referred to Pearson on several occasions, notably in his Everybody's political what's what? London, (1944). Here, on p. 246, Shaw speaks of Pearson as 'always smiling and charming' and reveals that he (Shaw) subscribed faithfully to Biometrika.
- 56 Pearson and Maria Sharpe were to marry, and to have three children, Egon, Sigrid and Helga.

- 57 Pearson Archive, University College London
- 58 Ibid
- 59 For details of Olive Schreiner, see, inter alia, A Calder Marshall, Havelock Ellis, London (1959), 88 - 105.
- 60 In 'The woman's question' reproduced in the Ethic of freethought.
- 61 Ibid, 384.
- 62 Ibid, 391.
- 63 Ibid, 394.
- 64 For an account of Stead and his work, see W.S.Smith, The London heretics, 1870 - 1914, New York (1968), 256. See also P.Keating, Into unknown England, London (1976), which gives a good sample of the social protest literature of Pearson's day.
- 65 For a sketch, see Smith, op.cit. (footnote 64), 76-83.
- 66 The text of Pearson's talk is preserved in the Pearson Archive, in the section given to the Men and Women's Club.
- 67 See G.M.Morant, A bibliography of the statistical and other writings of Karl Pearson, London (1939).
- 68 Ibid
- 69 This perception of Pearson has been developed by John Passmore in his Hundred years of philosophy, Penguin Books (1972)
- 70 For discussion, see E.S.Pearson, op.cit. (footnote 26), 20.
- 71 K.Pearson, The grammar of science 1st edn., London (1892).
- 72 See E.S.Pearson, op.cit (footnote 26), 22-23.
- 73 Pearson Archive, University College London.
- 74 Galton Archive, University College London.
- 75 See, e.g., Karl Pearson and Alice Leee, 'On the laws of inheritance in man. I. Inheritance of physical characters', Biometrika, 2, (1903) 357 - 462.
- 76 Speaker, 24 December, 1892.
- 77 In Phil.Trans.Roy.Soc., 187A (1896), 253 - 318.
- 78 See E.S.Pearson, op.cit. (note 26), 32.
- 79 Pearson to Galton Nov. 16, 1898 Pearson Archive, University College London.
- 80 In this context, see also chapter 7. See Pearson Archive for letter.
- 81 Ibid.
- 82 The early financial arrangements of the Galton and Biometric Laboratories have been discussed in Lyndsay Farrall's thesis.

The origin and growth of the English eugenics movement, 1865 - 1925
(Ph.D.thesis, Indiana University, Bloomington (1970) - copies available
from University Microfilms.

- 83 Pearson Archive, University College London
- 84 For this point, see D.W.Forrest, Francis Galton. The life and work of a Victorian genius, London (1974), 260.
- 85 Pearson Archive, University College London.
- 86 E.S.Pearson, op.cit. (footnote 26), 55.
- 87 For testimony to this effect, see, for example, the words of Stouffer, op.cit. (footnote 26).
- 88 The best general essay on Edgeworth is that in J.Maynard Keynes, Essays in biography, 3rd.edn, London (1951), 218 - 238.
- 89 F.Y.Edgeworth , 'The philosophy of chance', Mind, 4 (1834), 223-235.
- 90 F.Y.Edgeworth , review in Nature, 39 (1889), 603 -604.
- 91 A.L.Bowley, Edgeworth's contributions to mathematical statistics, London (1928).
- 92 Walker, op.cit. (footnote 26), 22 . See also in this context, H.M.Walker, Studies in the history of statistical method, Baltimore (1931)
- 93 For details of Yule, see A.Stuart and M.G.Kendall eds. The statistical papers of George Udny Yule ,London (1971). See also F.Yates, 'George Udny Yule, 1871 -1951', Obituary notices of Fellows of the Royal Society , 8 (1952), 309 - 323.
- 94 For discussion of this conflict, see Donald MacKenzie, 'Pearson and Yule on the measurement of association: A case-study in scientific controversy', forthcoming in Social studies of science.
- 95 See E.S.Pearson , 'W.S.Gosset, 1876 - 1937', Biometrika 30 (1939) , 214.
- 96 Ibid, 210 - 250.
- 97 For details of Greenwood, see Lancelot Hogben , 'Major Greenwood (1880 - 1949), Obit. notices Fellows. Roy. Soc., 7 (1950), 139 -154.
- 98 Pearson Archive, University College London.
- 99 Helen Walker, op.cit. (footnote 26),

Chapter 4. Karl Pearson and statistics: the cultural relations of scientific innovation.

Introduction

By now, something of the importance of Karl Pearson should be apparent. In the sense outlined, he created the new academic discipline of statistics, doing so in a context of biometry, actively linked with schemes to turn eugenics into an academic discipline. Concurrently, he was a successful philosopher of science - for his Grammar of science went through three editions.

In subsequent chapters, Pearson's influence will be further uncovered. But, by now, we have seen enough to realise that the emergence of Pearson and his biometric school raises a fascinating problem for those interested in the dynamics of scientific change. Several obvious historical problems have emerged; namely those of why Pearson should have responded so favourably to Weldon when approached, of why biometry should have led to statistics, and of why Pearson should have been so anxious to link statistics to eugenics. This latter integration was always real, and the Department of Applied Statistics founded by Pearson took its work style from Pearson's interpretation of Galton's desires¹

For Sir Francis there could be no safe progress in eugenics unless it was based on sound statistical theory, and on quantitative study of both heredity and environment. Such is the essential bond between the two laboratories.

In posing the historical problem of explaining Pearson's statistical endeavours, a further point should be added - namely that he constantly and actively promoted statistics as a universal methodology, capable not only of rendering eugenics scientific, but also of doing the same for a complete range of social sciences, including psychology, anthropology, sociology and craniometry. To the end of his days² Pearson emphasised the need to construct a research institute where his 'novel calculus' should be applied 'to living forms'. Thereby he meant a range of disciplines which incorporated, but which was not exhausted by 'pure' biology. So, to our list of things to be explained about Pearson must be added his conviction that statistics offered a universal methodology for the social sciences, and that it was his task to spread the new calculus.

In this chapter, I will attempt to develop a thesis of the following sort. Pearson entered willingly into biometry when presented with the opportunity by Weldon, not because of Weldon's

exceptional charm or because Pearson was short of projects or of work. He did so rather because he was an ardent ideologue and philosopher. He was a leading member of the nebulous association of 'social Darwinists', anxious to provide his Darwinism with a scientific basis and to show that Darwin and socialism were complementary, and not opposed, as frequently maintained by leading thinkers of the latter part of the 19th century. His conception of 'properly scientific', articulated in his Grammar of science and elsewhere, was such as to make it inevitable, or, at least, highly likely, that biometry's development, if at all forthcoming, would yield a harvest of statistical methods. Statistics, thus formed, embodied the central tenets of Pearson's philosophy of science, and, as such was to be universally recommended. It was to be applied to eugenics in particular, for this was a simple consequence of the aforementioned social Darwinian perspective. This perspective and Pearson's philosophy of science were, by turn, integrated components of a world-view constructed by a man attempting to come to grips with the social and intellectual problems, or 'contradictions' (or what have you) of late-Victorian British society. Thus, by many and subtle mediations, we may go from society to science. The thesis is developed ^{in several sections, and} I commence with a section entitled 'Biometry and statistics'. After providing intellectual and social background to the biometric movement, I attempt to show something of the way in which biometric problems led to the creation of the statistical techniques for which Pearson is so famous, and which were to form the core of the tuition within his biometric laboratory and Department of Applied Statistics. At this stage, the relation between Pearson's philosophy of science and his biometric and statistical endeavours should start to become apparent. We should be able to see by the end of the first section that the form that biometry took, and its role as the midwife of statistics, may largely be understood via its relations with Pearson's philosophical views. At this stage too, Pearson's espousal of statistics as an universal methodology should also become comprehensible.

The next section, entitled 'Science, Socialism and social Darwinism' addresses the further topic of why it was that Pearson was prepared to be interested in biology by Weldon ; for, biometry was, after all, a discipline devoted to the statistical solution of biological problems. It is one thing to explain the particular form taken by biometry and to show how it led to statistics on this account. It is another to explain why Pearson should have been prepared to enter into biological work. At the time it

was not a recognised or honoured path for the mathematician, and we may imagine that it did his career prospects little good. Certainly, his biometric work seems to have stood him in little stead in his 1897 application for the Savilian professorship at Oxford.³ The line I shall take is that of denying that Pearson ever was primarily interested in biology in its own right. I shall suggest rather, that he was essentially a social Darwinian - that is to say, one who believed that a scientific guide to social issues could be obtained from the philosophy of Darwin, suitably interpreted. His philosophy of science, I will suggest, enabled him to look with favour upon this position and also enjoined upon him the task of providing his Darwinism with a firm scientific footing. Pearson entered into biometry, into evolutionary biology, with a view to providing his ideology with such a scientific basis and with the hope of showing that a truly scientific Darwinism enjoined a move to state socialism rather than to the laissez-faire capitalism recommended by earlier writers on social Darwinism.

Hereafter is a third section, entitled 'Scenes from a Victorian life' in which I attempt to trace the development of the thought which predisposed Pearson to take up biometry in the manner outlined. Here I discuss not only his time in London, but also his earlier days in Cambridge and Heidelberg, attempting to trace the incidents and problems which were thrust upon him by the conditions of his life, and to show how his responses to these led him into the 'primed' condition which pre-disposed him to respond so favourably to Weldon and to start upon the major enterprise of his life - the building up of a biometric school of social biology and statistics. These explanations have their difficulties, and these are paraded at the conclusion of the chapter.

Biometry and statistics

(i) Background

Biometry was a construct of the England of the 1890's, and to an extent to be determined, reflected its background.

We have seen already that, by this time, rather little academic study of evolution had been carried out. Most academic work had been in phylogeny, and workers who suggested direct attacks on the mechanism of evolution - on heredity and the principles of variation, for example - had to defend their choices. Certainly, most of the theories of heredity afloat in the period in Britain - those of Spencer, Weismann, De Vries, Galton, Nageli and others - were the products of non academics or of foreign academics.⁴

Statistics, insofar as it was an institutionalised concern, was non-mathematical. The Statistical Society of London, set up in 1838, had little to do with mathematical statistics, despite the influence of Quetelet, and despite the papers of Edgeworth and Marshall the economists in the Jubilee volume for 1885.⁵

In social thought there are several relevant currents. The 1880's marked the onset of socialism.⁶ In 1881 Henry George came to England and promoted his views. In 1882 Hyndman founded the Social Democratic Federation, and 1883 saw the inauguration of the Fabian Society, which began issuing its tracts, and marked 1889 with the Fabian essays in socialism, edited by George Bernard Shaw. This was a non-Marxist group, that aimed its sights not at the targets of the Communist manifesto, but at a state turned socialist by the leadership of intellectuals and scientific administrators. All of this was played out against a growing recognition of the rottenness of urban England, notably of the metropolitan heart. 1883 saw the publication of The bitter outcry of outcast London, excerpted for W.T. Stead's Pall Mall Gazette, which revealed the conditions of an urban sub-proletariat,⁷ those who were to feature in Charles Booth's Life and labours of the people in London as the 'very poor'. 1890 saw the appearance of William Booth's In darkest England and the way out.⁸ 1884, 1886, and 1887 had large civil disturbances⁹

At around the same period, we find Bradlaugh making a reputation on the strength of atheism, Annie Besant facing prosecution for issuing a tract on birth control; an active freethought congregation at the South Place Chapel; a sprinkling of Comtean positivists, and good audiences for scientific publicists like Tyndall and Huxley.¹⁰

In the same decades various forms of social Darwinism were popular, and Darwin's ideas were invoked to support almost every conceivable form of social organisation. Andrew Carnegie and the great captains of American industry may have doted on the laissez-faire philosophy of a Herbert Spencer, but a Kropotkin, in his work in Mutual aid could also read into¹¹ the nature the message of the benefits to be derived from anarchic combination. Everyone had some opinion on Darwin and on the relation of his doctrine of the survival of the fittest to ethics and social policy, even if, like Huxley, it was to deny the relation's very existence.

In these streams, of course, there moved the venerable figure of Francis Galton, holding all of the views we have discussed, but not campaigning actively for eugenics much before 1900. Indeed, before 1900, eugenics was little more than a catalyst to research, but, afterwards, Galton was able

to deliver his message with some success - and no wonder when it is recalled that at that time there was also produced H.G. Wells' Anticipations of the reaction of mechanical and scientific progress upon human life and thought which both sold well and contained the following sentiment.¹²

It has become apparent that whole masses of human population are, as a whole, inferior in their claim upon the future, to other masses, that they cannot be given opportunities or trusted with power as the superior people are trusted, that their characteristic weaknesses are contagious and detrimental in the civilising future, and that their range of incapacity tempts and demoralises the strong. To give them equality is to sink to their level, to protect and cherish them is to be swamped in their fecundity.

13

As S. Hynes remarks, there is nothing in this passage that Baden-Powell would have boggled at. It is a familiar kind of turn of the century radicalism, 'mixing Darwin and Nietzsche and the idea of efficiency to compose a society that would be, in effect, an inhuman machine.' This inhuman machine was desired by several 'socialists' of the period.

(ii) Intellectual structures

Now, we may go on to consider biometry itself. Statements of the aims of biometry were common in the literature, but it may conveniently be regarded as a discipline which applied mathematics to the study of the variations occurring amongst the members of large populations, of their inheritance and of their responses to the pressures of selection. We have already seen Weldon's programme for biometric research, heavily endorsed by Pearson.

The statistical developments which these biometric investigations led to were nicely summarised by Raymond Pearl in his obituary of Pearson.¹⁴

1. The method of moments first employed simply as a device for curve fitting, but in the end, to have far-reaching consequences for the development of general statistical theory.
2. The system of skew frequency curves, as a technique for mathematically describing natural phenomena that individually vary.
3. The development of the theory of correlation and its application to the problems of heredity and evolution.
4. The χ^2 test for goodness of fit of theory to observations, coupled with the mathematical, logical, and statistical consequences and applications that grew out of it.

Now, one asks, could it be that the study of biology should have led to such results? The answer appears to reside in the circumstance that biometry was a branch of biology which stressed, as it had never been

stressed before in biology, the importance of exact measurement and of exact description, without theory, of the observable phenomena of evolutionary biology. In order to see this, let us take a particular example, namely that of the study of heredity - which led to massive developments in the theory of correlation. For, as Stouffer pointed¹⁵ out, Pearson was the 'perfector of simple linear correlation; inventor of multiple and partial correlation, of curvilinear correlation, of tetrachoric and bi-serial correlation'. The appropriateness of a discussion of correlation is further indicated by the introduction which Pearson offered to his statistics lectures, showing, as E.S. Pearson has noted, that for him, it was the idea of correlation which, stood as 'the fundamental illuminating conception of the statistical calculus'.¹⁶

"The purpose of the mathematical theory of statistics is to deal with the relationship between 2 or more variable quantities, without assuming that one is a single-valued mathematical function of the rest. The statistician does not think that a certain x will produce a single valued y; not a causative relation but a correlation. The relationship between x and y will be somewhere within a zone and we have to work out the probability that the point (x, y) will lie in different parts of that zone. The physicist is limited and shrinks the zone into a line. Our treatment will fit all the vagueness of biology, sociology, etc. A very wide science."

Galton, I have mentioned, developed the notion of correlation and that of regression whilst studying heredity in man - for, as is now well known, two generations of human stature follow, in good approximation, a binormal distribution. But, his statistical investigations went hand in hand with theoretical physiology, with a theory of inheritance based upon Darwin's 'pangenesis'. Pearson had no taste for a combined approach. Science, for him, was the stern business of observation and measurement, and stressed what we would now term 'operational definition'. The whole thrust of his approach may be gauged by the following extract from a key biometric memoir, of 1896.¹⁷

Heredity. Given any organ in a parent and the same or other organ in its offspring, the mathematical measure of heredity is the correlation of these organs for pairs of parent and offspring. The word organ here must be taken to include any character which can be quantitatively measured.

Pearson's goal was a purely phenomenal theory of heredity which related organs of parents and children without theoretical mediation of any sort - e.g., Galton's neo-pangenesis. And, given his chosen 'mathematical

measure of heredity' it is perhaps unsurprising that biometry should have led to the developments in correlation theory mentioned above. Let us take just a couple of examples, namely the development of ideas of multiple correlation and of tetrachoric correlation.

Multiple correlation

Pearson's first work on multiple correlation came in a paper of 1895, his third 'mathematical contribution', written on 'Regression, heredity and panmixia'.¹⁸ It was a paper which was designed to investigate the claim that the relaxation of selection would put natural selection into reverse. This view had a certain currency in contemporary biological literature, especially in the work of Weismann. And, of course, it had a certain parallel in Galton's observation that sons regressed linearly on fathers with a regression of about $1/3$. This suggested to Galton that the results of selection could never become stabilised, for, if an improved population deviating from the original mean by x ins. was allowed to breed without further selection, it seemed rational to suppose that successive generations would show deviations of $(x/3)$ ins., $(x/9)$ ins., $(x/27)$ ins., and so on. Pearson, as we shall see in a later chapter, had a variety of reasons for hoping to discredit this view. He was anxious to show that the offspring of a long-selected line of offspring would be immune from the tendency to regress constantly to the population mean when selection was relaxed. He hoped, in short, to show the power of selection to produce stable improved races or castes. We shall return to his motivation shortly,¹⁹ but, for the present, I would like to concentrate on the interplay between this biological ambition and its statistical consequences.

Galton, we have seen, knew how to deal with just two variables at a time. He was familiar with the joint (bivariate normal) distribution of parental and filial statures for example. Pearson now, in an attempt to construct a model allowing for the consideration of influence of ancestry more distant than the immediate parentage, developed an expression for the joint distribution of n normal variates, an expression, that is to say, for the n -variate normal correlation surface, expressed, as Eisenhart²⁰ puts it 'in a form that brought the computations within the power of those lacking advanced mathematical training'. As we shall see in the chapter on the law of ancestral heredity, the new formula had the following property. If one held that, jointly, the distribution of say, statures, in several generations of a population was adequately described by a multivariate normal correlation surface, then,

granted a knowledge of the various coefficients of correlation r_{ij} ($i, j = 1, 2, \dots, n; i \neq j$), and of the variances of the different generations, one could predict the expected value for the offspring of any selected line. That is to say, if one let x_1 stand for the filial deviate, x_2 for the parental, x_3 for the grandparental, and so on, then one could construct what we now call a multiple regression correlation giving the expected value of x_1 , given any particular set of ancestral deviates x_2, x_3 and so on. By these mathematical means, the long-continued effect of selection might be mathematically simulated. We shall see more of this in a later chapter, but ^{for} the moment it should be noted that Pearson's contribution to the theory of multiple correlation arose from his particular style of attacking the problems of heredity - a style derived from that of Galton, but, nevertheless, one differing from it in significant ways.

In the same paper, Pearson showed by a maximum likelihood method that, 'the best value of the correlation coefficient' (ρ) of a bivariate normal distribution is given by the formula now said to give the sample product-moment coefficient of correlation, generally known as Pearson's 'r'.²¹

Tetrachoric correlation

Having defined the correlation between, say, fathers and sons as the 'mathematical measure of correlation', Pearson was faced with the difficulty of knowing how to proceed in cases where there did not appear to be any normally distributed continuous variable involved. Thus, for example, he had data for sons and fathers in respect of their eye colour, and data for sires and foals in the case of horses' coat-colours, but was unable easily to measure the degree of correlation involved. These colours could not obviously be compared to normally distributed characters such as stature. All that one could do was to group the colours into broad qualitative categories - e.g., in the case of horses, 'bay', 'brown', 'chestnut' etc.

To overcome this problem, Pearson introduced the idea of 'tetrachoric' correlation.²² This was a method of calculating the correlation on a basis of a fourfold table which assumed that underlying the table there really was a bivariate normal distribution of say, the concentration of some pigment or other. Pearson's application of this method in his work, and, in particular, in some of his eugenic work, where he used it to determine the mathematical measure of the strength of heredity for human characteristics such as intelligence, was to lead him

into much criticism and controversy. But, the point for the present is that this well-known statistical technique for measuring the degree of association between two variables was first mooted in the context of a typically biometric problem situation - namely that of tackling the problem of heredity via a determination to measure the association between parental and offspring phenotypes (i.e., simple physical appearances). I will not pause to give details of the method, for these may easily be read in a standard text - in, for example, W. Palin Elderton's primer of statistics.²³

Other aspects

We see then, that Pearsons' massive developments of the statistical theory of correlation derived from his idiosyncratic and theory-free approach to the science of heredity. He wanted, in short, to be able to give mathematical measures of the strength of heredity and to make probabilistic predictions about the nature of the progeny of a line of ancestry without discussing underlying mechanisms of heredity. This theory-free approach was quite out of step with mainstream biological practice which was as much interested in the underlying mechanisms of heredity as in the business of predicting what the next generation would throw up. This tradition was not esteemed by Pearson. Its followers were like,²⁴

planetary theorists rushing to prescribe a law of attraction for planets, the very orbital forms of which they have not first ascertained.

It was in this way that the 'advance' of biometry led to advances in the theory of correlation. This, of course, is quite consistent with the circumstance that the mathematics, once embarked upon, presented problems which had to be taken up - e.g., that of the sampling distribution of the correlation coefficient. The point remains, however, that the search for a new science of heredity, a part of biometry, of a new mathematical approach to evolution, led to developments in statistical theory of a certain type.

Correlation looms large in Pearson's work - much too large his critics were to say. But, other notable features of his statistics also arose in a biometric context. We find everywhere the same emphasis upon the production of mathematical ways of describing observable phenomena, and on ways of checking up on the goodness of the description. Thus, for example, his first biometric paper was devoted to developing a method²⁵

for deciding whether a particular asymmetrical frequency curve found by Weldon when sampling crabs could be resolved as the sum of two normal distributions. His second paper²⁶ developed the series of Pearson curves as a way of describing non-symmetrical and unresolvable distributions of (biological) data. If the correlational part of his work stemmed from a desire for a way of offering theory-free accounts of the connections between different sets of data (notably those of heredity), then the aim in this other part of his work seems to have been that of finding ways of describing any given set of data, notably by fitting curves to it. (Thus, the method of moments.) Not all of Pearson's early statistical developments can be seen as the direct outcome of attempts to cope with biological problems. But they can always be seen as more general developments jibing with the aims for biology, and, generally, for science, noted already. The Chi-squared goodness of fit test,²⁷ for example, developed in 1900, is surely a good example. It is not that once we know Pearson's aims we are led instantly to the test. That is where his mathematical genius came into play. Rather, it is that, if we understand these aims, we understand his developing that kind of statistics.

Questions of method

The foregoing remarks about biometry's methodological style may be supported by going to texts. But, further confirmation for this perspective upon it, together with light on the origins of the style may be had from Pearson's methodological writings. These, I have noted, were for the most part contained in the three editions of his Grammar of science.²⁸ The claim here advanced is that given the aims and goals of biometry at the level of methodology - which we have seen above - we can see why biometry yielded statistics. Once one understands Pearson's philosophy of science, one sees immediately why it was that biometry had this methodological style, and why indeed, it is that his philosophy of science and his statistics must be seen as seamlessly joined.

In the editions of the Grammar, we find a philosophy of science which prefigures some of the views of the later logical positivist school. In a doctoral thesis on this subject, Riddle²⁹ has discerned three main components namely 'empiricism, a Kantian emphasis on the role of the mind in organising and interpreting sensation, and a Cartesian faith in mathematics as the key to organised scientific thought'. The Grammar, Riddle notes, is 'largely an attempt to impress the ideas of Mach upon the English speaking world'.

Riddle's diagnosis seems entirely correct. Pearson held that the only sources of knowledge were the data of sensation. Physical objects, other minds, the theoretical concepts of science and so on therefore had to be seen as logical constructs out of sense data. For Pearson, the circumstance that they were constructs was paramount. What cannot be known, he stressed frequently, is whether or not these constructs corresponded to reality. He was much preoccupied with the Kantian Ding an sich - the 'thing in itself' - which he portrayed as falling into the realm of the fruitless discipline of metaphysics. We cannot know the Ding an sich, he argued, all we can know are the inputs of experience, variously refined and colligated by an active mind. Indeed, for Pearson, the mind was a potentially very active factor in the construction of personal reality.³⁰

It may be the perceptive faculty itself, which, without being directly conscious of it, contributes the ordered sequence in time and space to our sense impressions. The routine of perceptions may be due to the recipient and not characteristic of the material.

The main feature of Pearson's cosmos, then, was its sheer unknowability. All that could finally be asserted was the Kantian claim that,

it is a necessary condition for the existence of thinking beings that there should be a routine of perceptions.

But, since 'the necessity thus lies in the nature of the thinking being' (a non-sequitur, but no matter),³¹

it /the routine/ is conceivably a product of the perceptive faculty.

Any connection, through experience, between the self and the real world was highly tenuous. Of the world outside of sensation, said Pearson, 'science can only logically infer chaos, or the absence of the conditions of knowledge'. What then, for him, could be the rational goals for science? None other than the discovery of scientific laws, which described the flow of appearances, or, more precisely, the regularities therein discernible. These laws did not explain - for the 'why' of things was an immutable mystery - and they could certainly not explain by referring the observable to the unseen operations of underlying realities, as was commonly supposed by other scientists, who referred the visible realm to an invisible realm of atoms, molecules, and so on. There were no such realities, or, at least, no such knowable

or guessable realities. All that science could do was to uncover laws that were summaries of observed regularities, instruments of prediction. Their rationale lay in their utility; they stepped up the human survival potential in the struggle for existence, and did this best when they had the economy and the precision due to expression in precise mathematical form. Biometry was the application of this philosophy to the biological realm. The multiple regression equations used to predict offspring phenotypes on a basis of knowledge of ancestral phenotypes were intended by Pearson as the exemplification of true science, not of a science that mistakenly tried to get down to the underlying causes of things. Biometry was a natural Pearsonian research programme and, it should now be clear, the statistics emerging from it have to be seen as the mathematical expression of his philosophy of science. Good Cartesian that he was, it gave him a mathematical way of economically describing the flow of appearances, in the non-physical sciences. But, Kantian as he was too, statistics, notably the theory of correlation, had a greater significance still for Pearson. He felt that, in correlation³² theory, he had the makings of a profound philosophical revolution. Kant had insisted that the world, to be intelligible, must, without exception, conform to a rigid determinism. Pearson claimed that Kant had the category wrong. Experience, to be intelligible, must, and in fact did, conform to the category of correlation. As seen above,³³ it was his view that y was never a single valued function of x , or of x_1, x_2, \dots, x_n , for any value of n . The Universe, possibly the product of the human mind, could be adequately dealt with only by correlational methods; methods which recognised and coped with its inherently non-deterministic nature.

Within such a world view, the techniques of correlation developed in the biometric laboratory attained a very high significance. Note, for example, the view of Ethel Elderton, a Galton Laboratory employee, when discussing research methods for the social sciences.³⁴

What guide can we take to indicate the path of true social reform through such a tangle of cause and effect as we find involving the relative influence of nature and nurture on human life? It is not enough to show that results are associated with this or that factor; we have a vast complex of associated factors, and out of this complex we have in some way to pick out the more important and in a certain sense the fundamental factors. The only effective method by which at present it seems possible to approach such a problem is that of correlation. Taking the social conditions we wish to modify, we must study their correlation with as many factors as we can possibly measure. In the choice of these factors, we must of course be guided by the reasonable probability of association

and by the limits of human life and energy. The correlation of a multiplicity of factors being known, we may justifiably assume that the factors with the highest correlations are, among those dealt with by us the most important and then the process of 'partial correlation' will guide us still further towards a final judgement of what fundamentally are social cause and social effect.

Pearson's statistics, therefore, arose out of his biometry, whose form was dictated by his philosophy of science, which, by turn, invested his statistics with a high philosophical significance - notably as a mathematical and sound methodology for making the non-physical sciences truly scientific. Lancelot Hogben, I think, spotted this aspect when he spoke of the 'Pearsonian evangel' which would extend 'mathematical free grace' to the social sciences, but at Pearson's positivistic price.³⁵

So far then, I hope to have established that Pearson's style of doing biometry arose directly out of his philosophy of science. (In a fuller discussion of the effects of philosophy on 19th century mathematical biology, one could perhaps fruitfully compare Pearson's work with that of the Aristotelian D'Arcy Thompson, and his work on Growth and form.)³⁶ The methodological style of biometry, by turn, may be cited as explaining biometry's power to produce new statistical techniques. What remains unexplained, of course, is Pearson's preparedness to suddenly embark upon a life of biologically orientated research when approached by Weldon in the early 1890's. This matter I will now address.

SCIENCE, SOCIALISM AND SOCIAL DARWINISM

It might be suggested that Pearson's responsiveness to Weldon needs little explanation - for, Weldon presented him with a set of stimulating problems and, of course, Pearson had already said a little on the calculus of probabilities in his early Gresham lectures.³⁷ In response, I would suggest that the early Gresham lectures do not have the appearance of the first steps in a career devoted to statistics, and that the magnitude of Pearson's response to Weldon suggests strongly that he must have been strongly predisposed to enter Weldon's line of country. We should not forget the complete way in which the uptake of biometric problems changed Pearson's intellectual life - or, at least, appeared to change it. One guide to this is given by Pearson's publications. In the official bibliography of his works,³⁸ the period to 1894 includes 55 items listed

as 'Literary and historical'. Thereafter, Pearson published only 10 further items so classified. 1894, of course, is the year of publication of his first biometric paper. The period after 1894 contained 405 further items listed in Pearson's ^{bibliography} 'Statistical'. The section headed 'Pure and applied mathematics and physical science' contains 4 items in the period to 1884 and 32 thereafter - suggesting a more or less uniform rate of production in this area. In short, there can be few more amazing turn-about in the history of science. It is this turn about that I now address, but not before stressing that it would be wrong to see Weldon's role as an overly simple one. For, before his meeting with Galton, Pearson had already encountered Galton's ideas and methods, speaking on Natural inheritance to the Men and Womens Club shortly after its publication in 1889. In his talk, Pearson offered a less than fulsome account of Galton's methods ³⁹

Personally I ought to say that there is, in my own opinion, considerable danger in applying the methods of the exact sciences to problems in descriptive science, whether they be problems of heredity or of political economy: the grace and logical economy of the mathematical processes are apt to so fascinate the descriptive scientist that he seeks for sociological hypotheses which fit his mathematical reasoning and this without first ascertaining whether the basis of his hypothesis is as broad as that human life to which the theory is to be applied. I write therefore as a very partial sympathiser with Galton's methods.

And, in his copy of the work, Pearson pencilled in his exasperation with Galton's style of argument. On page 30, for example, he commented on Galton's analogical arguments as follows ⁴⁰

It is merely an analogy without any scientific value as to the how still less the why.

Yet, later on, Pearson recalled that he had interpreted the introduction to Natural inheritance to mean that ⁴¹

... there was a category broader than causation, namely correlation, of which causation was only the limit, and that this new conception of correlation brought psychology, anthropology, medicine and sociology in large parts into the field of mathematical treatment. It was Galton who first freed me from the prejudice that sound mathematics could only be applied to natural phenomena under the category of causation. Here for the first time was a possibility - I will not say a certainty of reaching knowledge - as valid as physical knowledge was then thought to be - in the field of living forms and above all in the field of human conduct.

Clearly, Weldon acted as a middleman, able to reinterpret Galton's

thought in a manner more attractive to Pearson. We have already seen something of Weldon's positivistic approach to biological problems, and in this chapter we see that this style, so different to Galton's, was one that harmonized with Pearson's views.

Darwin and society

It is good to start by recalling that Pearson's philosophy of science was also a philosophy of life. Just as, say, Karl Popper's scientific epistemology comes conjoined in various fashions with views on the proper conduct of life and society,⁴² so did Pearson's. Indeed, it would have been strange if it did not, for there must be some relation between what one thinks about the nature and possible scope of scientific knowledge and the ways in which one supposes that affairs in general are best directed. Pearson always made it clear that knowledge and ethics marched hand in hand. For him, it was ethical to know. Science was the best form of knowledge, and, when it advanced, metaphysics and religion retreated, to be replaced by firmly established positive knowledge, which, being confined to the phenomena, avoided the dangers inherent in going beyond the phenomena. The ideal man for Pearson, the late-Victorian anti-religionist, was the free-thinker, whom he celebrated in his book the Ethic of freethought.⁴³ The freethinker had knowledge, 'truth' in fact. He would have 'assimilated the results of the highest scientific and philosophical knowledge of the day'. He would be a sound citizen, trained in science's 'impersonal judgement' techniques. He would be able to assess, for example, the views of Weismann and to employ his judgement when thinking about the right conduct of society to its 'anti-social members', which would remain an open question until one knew 'what science has to tell us on the fundamental problems of inheritance'. It was not, however, that a correct knowledge of the continuity of the germ plasm would, per se, illuminate this social issue. The naturalistic fallacy was a component of Pearson's philosophy of science.⁴⁴

Each one of us is now called upon to give a judgement upon an immense variety of problems, crucial for our social existence. If that judgement confirms measures and conduct tending to the increased welfare of society, then it may be termed a moral, or better, a social judgement. It follows then, that to ensure a judgement's being moral, method and knowledge are essential to its formation. It cannot be too often insisted upon that the formation of a moral judgement - that is, one which the individual is reasonably certain will tend to social welfare - does not depend solely on the readiness to sacrifice individual gain or comfort, or on the impulse to act unselfishly: it depends in the first place on knowledge and method. The first demand of the state upon the individual is not for self-sacrifice, but for self-improvement.

For Pearson, as for Clifford,⁴⁵ knowledge was next to goodliness. But goodness was linked to the idea of the 'increased welfare of society'. What was that? And, why should Pearson have regarded it as a criterion for moral action?

The resolution of this issue is simple once we grasp fully all the things hinted at in the passage from the Grammar cited above. Take, for example, the question of what Pearson termed the welfare of society. When we look at his work, it becomes increasingly clear that what he has in mind is not something like the maximisation of happiness, but something related to much sterner criteria, amongst which are numbered items such as national survival and supremacy in the international struggle for existence. Pearson is known to social historians as the key promoter of 'external' social Darwinism - of the doctrine that the correct way of envisaging the struggle for existence in human affairs is not at the level of man against man, but at that of nation or race against nation or race, with success in the struggle going to the best equipped and organised group. In the Grammar, alongside the epistemological doctrines, this is all laid out. We learn of Pearson's conviction that history⁴⁶

can never become a science, can never become anything but a catalogue of facts rehearsed in more or less pleasing language, until these facts are seen to fall into sequences which can be briefly resumed in scientific formulae. These formulae can hardly be other than those which so effectively describe the reactions of organic to inorganic and of organic to organic phenomena in the earlier phases of their development. The growth of national and social life can give us the most wonderful insight into natural selection, and into the elimination of the unstable on the widest and most impressive scale.

But, it is not only political history that will receive a new scientific basis in Darwin's work, for,⁴⁷

the two great factors of evolution, the struggle for food and the instinct for sex will suffice to resume the stages of social development.

It is in this context that socialism emerges on the scene, though it is a socialism more akin to that of H.G. Wells⁴⁸ than those of Marx or even of Attlee. Pearson attacked violently the idea, ascribed by him to Ernst Haeckel, Herbert Spencer and T.H. Huxley, that Darwin and socialism were incompatible. On the contrary, he averred, socialism, by which he meant the 'tendency for social organisation, always prominent in political communities', was a factor of great importance in the evolutionary struggle, for it led to success, on the part of the group that enjoyed this organisati

to success in the 'intense struggle which is ever waging between society and society'. In fact, the lesson of history was the lesson of socialism, and science would ultimately balance 'the individualistic and socialistic tendencies in evolution better than Haeckel and Spencer seem to have done'. Certainly,⁴⁹

in the face of the severe struggle, physical and commercial, this fight for land, for food and for mineral wealth between existing nations, we have every need to strengthen by training the partially dormant socialist spirit, if we as a nation are to be among the surviving fit.

This new pattern of organisation, he considered, in 1892, must 'largely proceed from the state'.⁵⁰

Here it is that science relentlessly proclaims:
A nation needs not only a few prize individuals;
it needs a finely regulated social system - of
which the members as a whole respond to each
external stress by organised reaction - if it is to
survive in the struggle for existence.

Socialism, Pearson repeatedly asserted, was a system of morality, with the state as its central object. The individual, he said, would have to learn to say, with Louis XIV 'l'etat, c'est moi' and even be prepared to give 'short shrift and the nearest lamp post' to offenders against the state.

One could go on, but the gist of things should now be quite clear. Pearson's doctrines are far from entirely clear - interestingly, even Weldon regarded him as a muddled thinker outside of the realm of mathematics - but included the following positions, held before his meeting with Weldon and his move into biometry.

- (i) History is an important subject. But it is to be understood in terms of the principles of Darwinian evolution. At this stage, it becomes a science - a biological determinism to rival historical materialism.
- (ii) In important practice, the struggle for existence in history goes on between group and group, with differing patterns of social mores waxing and waning according to their power to assist the group in its struggles.
- (iii) The ultimate legitimation of morality has to be sought in the biological standard of group survival. Only with a people attuned in their outlook, showing Clifford's 'tribal conscience', could there be built up a society with 'permanent stability'.

(iv) On scientific grounds, therefore, the proper goal for the members of a society is the production of 'a finely regulated social system' enabling it best to survive in the struggle for existence, and to be 'among the surviving fit'.

Now, surely, we begin to see why Pearson undertook biometry, mathematical evolutionary Darwinism, so easily, and why his efforts to establish eugenics as a discipline must be seen as allied to, rather than as separate from his biometry.

Pearson, we see, was an extreme freethinker, who sought after guides to morality and social policy in the discoveries of science, notably within the ideas of Darwin. It is no surprise that he wrote with feeling of the 'ethic of freethought'. In biometry, clearly, Pearson could achieve several agreeable ends. He could produce an exemplar of the 'metaphysics - free' style of science which was enjoined upon him by his philosophy, and, at the same time provide solid foundations for the Darwinism upon which he based morality and social policy in general and Pearsonian state-socialism in particular. Within this state-socialism, with its emphasis on group fitness and group survival, an interest in and a commitment to eugenics was hardly surprising. These views may be supported by a good deal of evidence, in the shape of views openly proclaimed by Pearson at the time of his biometric writings.

We may take, for example, the article which Pearson published in the Fortnightly review for 1894,⁵¹ where he attacked Benjamin Kidd's successful Social evolution.⁵² Kidd had argued, against Marx in particular, that all forms of socialism limiting in-group competition would put an end to social progress, a view often linked with the aforementioned doctrine of 'panmixia',⁵³ which held that 'without natural selection degeneration must set in as certainly as death follows life'. Pearson, naturally, did not reject the general idea that evolution gave a guide to life, but attacked the quality of Kidd's evolutionary premises, which converted 'evolution' into a 'cant term to cover any muddle headed reasoning, which would utterly fail to justify itself had it condescended to apply the rule of three' - a fault ascribed to Spencer, Huxley and Haeckel too. Against them must be opposed the new biomathematical logic already embarked on by Galton and Weldon. As yet, the results of the new approach were only beginning to appear, but in the meantime, Pearson recommended the following position:⁵⁴

That until the quantitative importance and numerical relationships of various factors, vaguely grouped together as the theory of evolution, are accurately ascertained, no valid argument can be based on the theory of evolution with regard to the growth of civilised human societies. We must remain agnostic as to these problems until the theory of evolution has been readjusted on its new basis.

Generally, it was 'quite possible that the socialist movement will react on biological science as it already has done on economic science'. This would be done by the mathematical investigation of anthropological data with a view to discerning the 'relative numerical importance of the several factors of natural selection'. Only when these investigations were complete would it be time to 'talk about the antagonism of socialist theory to biological laws'. A socialist like himself could express judgement on biological issues because the key questions, in the last resort, were mathematically approachable, and must lead to 'the theory of evolution becoming a branch of quantitative science'.

Here then is the justification for a mathematician, however limited his range, interfering when he observes biological principles, first stated without any quantitative theory or statistical bases, and then adopted as valid arguments in dealing with the great social problems of our time.

Pearson, by then, had published many articles advocating socialism of the sort already indicated, which stressed the need for hierarchical 'socialist' organisation to produce success in the struggle between nations, a success which depended crucially on the intellectual classes. As he put it in 'Socialism in theory and practice, being a lecture delivered to a working class audience',⁵⁵

You must never forget how much of that organisation, that education, is due to labourers with the head. Some of you may be indifferent to the great empire of England, to this superiority of Englishmen, but let me assure you that, small as . . . in some cases is the comfort of the English working classes, it is on the average large compared with that of an inferior race - compared, say, with the abject misery of the Egyptian peasant.

Pearson believed fully, by 1894, that his own work on heredity, fertility and selection would show, contra Spencer and others, that laissez-faire society did not eliminate the socially unfit, but encouraged their fecundity. Only the socialist state, dedicated to national fitness,

could remedy this. Reading between the lines, we can see, even at this early stage that Pearson's socialist state would also be an eugenic state.

The general point of the tie-up, in Pearson's mind, between socialism, historical understanding and biometry, with the former producing interest in the latter, may be taken by looking to a letter Pearson sent to the Manchester Guardian in 1901, replying to a leader of the previous day, which had denounced his recent work on National life from the standpoint of science,⁵⁶ a gloomy and aggressive jeremiad which had presented a 'scientific view of a nation', as ⁵⁷

that of an organised whole, kept up to a high pitch of internal efficiency by insuring that its numbers are substantially recruited from the better stocks, and kept up to a high pitch of external efficiency by contest, chiefly by way of war with inferior races, and with equal races by the struggle for trade-routes and for the sources of raw material and of food supply. This is the natural history view of mankind, and I do not think you can in its main features subvert it.

In his letter, Pearson, anxious to rebut the charge that he was just another politically ignorant biologist turning his microscope to the world of affairs, with the usual disastrous consequences, defended himself in a series of rhetorical questions (a favourite device of his) which indicated his own intellectual path to biometry. What grounds, he inquired, did the editor have⁵⁸

for supposing that I may not have spent more years of my life in historical work than in the study of heredity; that I may not possibly have laboured more carefully at history than at biology; that more of my published work may not deal with the former than with the latter; nay that even my endeavour to understand something of inheritance and of racial struggle may not have arisen from my attempts to read history aright? May it not be that I am convinced that through the principle of evolution by natural selection combined with inheritance, light alone can be thrown on that maze of wars, movements, national survivals and national decays which passes for history in our current

text books? Is it not just possible that a man who has thought and worked in the historical field may have turned to the biological field because he has been driven by the force of facts to see that the keynote to the history of man lies in the struggle for food and in the struggle to reproduce, which are the great factors at the base of all biological reasoning with regard to the development of animal life I ask what reason you have for supposing my history an outgrowth of 'biological consciousness', rather than that my interest in heredity has arisen from my conviction of its bearing on historical studies?

Now, some of the most typical patterns of Pearson's biometry should be understandable - e.g. its strong emphasis on inheritance (often of psychical characters) and its constant association with discussions of the relative fertilities of different groups in the population,⁵⁹ and of the possibility of national deterioration through laissez-faire in reproduction, leading to restraint on the part of the professional classes and to proliferation on the part of the lowest orders. A truly Wellsian fear! Certainly, Pearson had little difficulty in accepting tie-ups between social position and natural ability. It was a point of high significance ^{for} of him.⁶⁰

The general conclusion is that Pearson's interest in biometry, and its heavy emphasis on heredity, was systematically related to his prior commitment to a certain type of social Darwinism upon which he had antecedently built a defence of socialism and an account of the moral act. Within this outlook, issues of eugenics rose in a hard, strong fashion. For, if the struggle between groups was the dynamic of history, and if morality was the study of agencies making for group survival in this struggle, then the questions of eugenics became crucial. In this context, it became mandatory for Pearson to have a scientific account of evolution, heredity and selection; for they were the agencies deciding the fates of nations. A proper scientific approach was always called for by the ^efreethinker, and, furthermore, Pearson was certain that such an approach would yield results destructive of 'anti-socialistic' forms of social Darwinism. A great deal, of course, hinged on his readiness to accept seriously Galton's principle of the like heritability of mental, moral and physical characters. But, as we shall see, by the mid 1880's, Pearson was well disposed to take this view seriously. Having done so, the terms of his rather composite philosophy demanded that he pursue it doggedly.

Thus it is that we understand Pearson's : brisk , response to Weldon's approach, and his attempt to construct a new type of biology. It was a type, a style, dictated by a distinctive philosophy of science, and one productive of statistics. Biometry was a many faceted discipline for its constructor - a truly scientific formulation of the principles governing society, history and morals. Its by-product, statistics, was, by turn, the correct methodology which the freethinker would apply in all domains when desirous, as he had to be, of scientific knowledge. Biometry, statistics and eugenics were seamlessly joined, with one another and with a philosophy of life.

SCENES FROM A VICTORIAN LIFE

Introduction

So far, we have seen how Pearson's statistics arose naturally from the complex of views he had adopted by the early 1890's - views, that is, on the philosophy and social functions of science, on history, socialism and Darwin. In what follows, I shall undertake to explain how it is that he came by these potent beliefs by looking at selected episodes in Pearson's earlier life and by seeing him reacting to his Victorian circumstances.

Cambridge

To find the roots of Pearson's philosophy of science and social Darwinism, we should look back to his student days. At Cambridge, he was a member of King's College,⁶¹ then a small community where dons and students mingled well. Here, he met Robert Parker, the future law lord, Henry Bradshaw the librarian, Macaulay the mathematician and Oscar Browning the historian. Then, as ever, he looked for a few close friends, and, in King's was rewarded by, in particular, the friendship of Parker.

At Cambridge, however, Pearson was not carefree. His 'commonplace book' for 1877⁶² suggests a state of turmoil, which led him to a piece of self-analysis in which he attempted to clarify his views on religion 'till I was left with some definite idea of what religious belief I have or whether I have any at all'. The answer was vague and rambling, but showed his dislike of Victorian laissez-faire society and his religious difficulties, not uncommon amongst young intellectuals in the post-Darwinian period. Think perhaps of the young Alfred Marshall undergoing⁹ a crisis of religious faith in the late sixties, and the parallel

defections from Christianity among men of the calibre of James Ward, Leslie Stephen, Henry Sidgwick and W.K. Clifford,⁶³ Pearson, though, unlike Clifford and companions, was not connected with groups like the Grote Club or the Metaphysical Society. His was a lonely vigil, and, in his book, he wrote that he had rejected Christianity, and would hold to deism so long as he was in contact with nature, but not when he saw men in 'rage and tatters' in towns. He could not understand why God had not enabled the average man to read Goethe and Shakespeare and lose himself in 'that heaven of bliss, intellectual sensualism'. Turning to secular nostrums, the scene was equally poor. Progress, utility and the 'welfare of our race' were all rejected as possible life-goals. In the end lay the existentialist's question:⁶⁴

What am I placed here for, what are my duties, how
shall I know them if there is no revealed religion,
no right, no wrong?

Pearson, in short, was a candidate for philosophy (as had been Clifford and Marshall before him at a similar period), and his writings of the period portray him as engaged in searching for some substitute for the Christianity he had relinquished. In the language of the *times*., he was searching for a creed, something non-religious on which he could focus his religious feelings. This comes out everywhere; for example, when he wrote to Parker about Carlyle's Sartor resartus.⁶⁵

You are always blaming me for my love of the ideal
when it has no application to the practical..but,
somehow, since all my religious dogmatic faith fell
to the ground, I feel that I can only be happy by
adding a mystic ideality to everything and looking
at everything from a religious point of view. This
does not add to, rather impeded my practical action,
but it does supply a want I feel. It is this spirit
of the ideal which Carlyle tries to cast over
everything and which delights me so.

His non-mathematical reading at this time was chiefly in British empirical philosophy and in German literature - in Goethe, Herder, Schelling and others. Like Carlyle he was an enthusiast for Goethe's Wilhelm Meister. His concern for philosophy was real enough. In February 1879 he read Berkeley's works, and, at about the same time, again like Marshall before him, decided to go to Germany to study philosophy - to Heidelberg in fact, to work with Kuno Fischer. With this philosophical interest, sensu strictu,

was combined an interest in the works of the 'scientific publicists', Clifford, Huxley and Tyndall.⁶⁶ Thus, in letters about the Haeckel-Virchow debate, we find him proposing Haeckel as the German Clifford, and referring to the 'old battle' - between science and religion. The main point, perhaps, is that Pearson appears to have emerged from Cambridge as something of a lost soul - as one with a strong desire for a creed, for somewhere to focus the religious impulse which, on his own acknowledgment, he was highly prone to. His state of mind at the time is indicated by verses in a commonplace book kept at this period, referring to the life of 'Arthur'.

At other times with chosen friend or two
He discussed Nature, God, the right of kings,
Freedom, the rule of many or of few,
The starry vault and what per se are things.
Spinoza's "Intellectual Love" oft rings
Within their ears; they all those theories sound
Of death, life, soul, with which the springs
Of complex philosophic thought abound.
They strive to know the All, yet never find firm ground.

Arthur, clearly, is Pearson. And, though no poet, we see in Arthur's struggles the sorts of difficulties with which Pearson felt himself confronted; problems which, in one form or another, were widely felt (though often more precisely put) in the post-Darwinian period. He was a man who needed a new philosophy of life.

Heidelberg and London

In Heidelberg, in 1879, Pearson hoped to find this new philosophy, this new creed as he would have put it. Here, he studied philosophy with Fischer, and physics with Quincke,⁶⁸ later recalling his experiences, in semi-autobiographical style, in his New Werther,⁶⁹ a modern variant of Goethe's original, which, despite its deliberately gushing style, tells a great deal about Pearson and parallels the 'Arthur' verses of the same period. These reveal his loneliness and despair, and his decision to turn to Germany, the 'country of ideas' and his love of the German - a love to be reflected in the change of name from Carl to Karl, and in his occasional open regret that he had not been born a German. In Germany he seems to have been prey to an occasional mild nature-mysticism, and to have kept the company of Raphael Wertheimer, a Jewish law student and radical, who features prominently in the New Werther.

Morbid he shunned his fellow countrymen,
 At best a heartless pleasure-seeking crew.
 (With more of vice than virtue to my ken)
 Of trusty friends he could but number two:
 The first a student of the law, a Jew,
 Was atheist, socialist, and loved
 State-theories to propound, so false and true.
 The second Gaspar, faithful hound, who roved
 Where'er his master went, beyond all else beloved.

In the New Werther, Wertheimer is portrayed as introducing Pearson to socialism, saying of the English that they⁷⁰

do not recognise the difference between a French community, a Russian nihilist, and a German social democrat, but brand them with a common stigma as subverters of society.

Wertheimer, a social democrat, insisted that⁷¹

We do not wish a revolutionary change in all old laws and customs; we recognise the truths which history has taught, that real change is gradual, and yet also that change is necessary to life. The violence of some persons claiming to be members of the party is due to the ignorant and the vicious whom the leaders cannot prevent from joining their banner. You must distinguish 'Katheder Socialismus' - Marx and Wagner from 'Gassen Socialismus', as represented by Hödel and Nobiling.

Clearly, Wertheimer found a convert of sorts in Pearson, who thereafter proclaimed himself a socialist - though, as we have seen, his socialism was a form of elitist state-socialism which does not enjoy much popularity at the moment. In all his writings, Pearson conjoined a desire for change with a fear of anarchy; this has been seen in his writings on anarchy, which ended with demands for a new society in which power would be related to intellectual capacity rather than to financial standing. An indication of how Pearson's thought on social matters developed in Germany is given by a paper of 1881 on 'Political economy for the proletariat',⁷² which attacked traditional political economy and compared the 'individualism of Bentham' unfavourably with the 'socialism of Fichte'. Fichte, it would seem, played a large part in Pearson's thinking about politics. We know that he was an author studied by Pearson, who quoted with approbation.⁷³

Lastly Fichte arriving at the conception that every species has its purpose, argues that if, in the development of mankind, the purpose of the species man is ever to be realised, so must all individual forces be united and directed to this one purpose The state is the real expression of the species, and its purpose is the purpose of the species. The state demands the forces of all individuals alike and all their force. Of what use is any force to the state, if it does not serve the state? The purpose of the isolated individual is enjoyment, the purpose of the species is culture. What does not serve the purpose of the state, does not serve the purpose of culture, the education of the whole, not even self education, but runs rankly to weed in barbarism. What a gulf separates the individualism of Bentham from the socialism of Fichte! The reader may perhaps ask what is the aim of prefacing so many opinions to my definition of the purpose of a state? Only this - that a definition from its very nature cannot be based on logical proof, and I wish thereby merely to prepare the reader for my own view of the matter, which he must accept or not as he feels its truth or falsehood.

I shall define, therefore, the true aim (i.e. duty) of the state as the perfecting of mankind. All other ends are merely auxiliary to this, and this statesmanship. Let it be noted that I use the term perfecting and not perfection.

In the new order, traditional political economy would give way to a new science that would,

consider the duties of the state in regard to its ideal - the improvement of mankind - and since the measure of existing culture in a state must approach rather that of its lowest than its highest members, to consider peculiarly the duties of the State with regard to the Proletariat, to examine the state subjectively - its most suitable form under given positive circumstances, and the relation of the subjects to the actual executive - will form the political branch of this science; to examine the state objectively, its relation and duty to its subjects will form the ethical side. All the ordinary categories of political economy - capital, labour, land, trade and so forth - must be judged from this new standpoint, and I fear not a few of the results attained will be found to differ from the mammon-worshipping doctrines of Ricardo and his disciples.

The nearest extant approach to all of this, said Pearson, was to be found in Germany, in the Katheder-Socialisten, who under Schmoller, helped frame Bismarck's social policies.⁷⁴ In particular, Pearson singled out the words of Held, citing his demands with full approval.

They demand above all that the premise, that the individual man in economic affairs is ruled only by egoism be abandoned, and they contest the principle that man shall unconditionally be ruled

only by egoism as thereby the general welfare is most surely advanced. On the contrary they assert that public feeling is always effective at the same time as egoism and shall be so - ethical political economy. Finally they demand that the economic man must also be considered as a member of a state organism, they reject the suggestion of an unusually valid natural law, and demand that each existing judicial system must in whole and part be considered critically as a factor of the greatest importance, in the formation of economic relations - the politics, social, historical, judicial standpoint.

It seems, therefore, that, in Germany, Pearson picked up --an outlook based on the historical tradition; one which is perhaps best described as Spenglerian - his writings remind one⁷⁵ of Spengler's ^{later} approbation of the 'Prusso-socialistic state', which,

.. is the whole folk, and over against its unconditional sovereignty both /the bourgeoisie and the proletarian/ are merely parties - parties, minorities; both serve the commonalty.

Pearson's writings stressed heavily the desirability of an organic state, whose ranks and grades were bound by ties of common purpose. The citizen, he was to write, must see socialism as a new morality; he must be prepared to say, along with Louis XIV 'l'etat, c'est moi'. So far, little has been said about Darwin, but surely, it is easy to see the links between the social views described above and the external social Darwinism which Pearson was to adopt. The goal for the state became survival in the international struggle for existence, and in this 'external' social-Darwinian view, a hierarchical collectivism, a socialism could be defended as that which optimised the chances of survival. Morality, similarly, could be tied to the state's good. It seems surely, that Pearson was first attracted by a variant of German social philosophy, in an age when the relation between Darwin and society was a major topic. He was able to accommodate his standpoint within a Darwinian outlook after the manner seen - a manner opposed to the English, individualistic Spencer, which treated of socialism, the principles of history and many other things too. It was an accommodation swiftly achieved. By 1885, in a paper on the 'woman's question', Pearson was writing thus:⁷⁶

It is the complete disregard of sexualogical difficulties which renders so superficial and unconvincing much of the talk that proceeds from the 'woman's rights' platform. We have to first settle what is the physical capacity of woman, what would be the effect of her emancipation on her function of race-reproduction, before we can talk about her 'rights', which, are, after all, only a vague description of what may be the fittest position for her, the sphere of her maximum usefulness in the developed society of the future.

By 1886, the move was complete. In his paper on 'Socialism and sex', Pearson noted that 'there is a principle lying at the basis of all growth, which was first made manifest by a naturalist' but which would 'one day receive its most striking corroboration from the scientific historian'. And, in a note, he argued that Herder's metaphysical philosophy of history naturally failed, that 'the philosophy of history is only possible since Darwin' and that the rationalisation of history by the 'future Darwin' would consist in 'the explanation of human growth by the action of physical and sexualogical laws in varying human institutions'. A romantic German historicism had been replaced by an English, Darwinian one. This expulsion of metaphysics was, of course largely illusory, about as real as was the scientificness of Pearson's 'scientific view of a nation'. (See above). This, however, hardly matters from the historical standpoint. What is of interest is the way that Pearson's social Darwinism and its connection in his mind with socialism, which underlay his move into biometry, came into being.

Why Pearson should have been so ready a convert to genetic determination of human mental traits is hard to say, though, as an admirer of Goethe he was certainly familiar with Goethe's view that there was more intellectual divergence between the highest and lowest man than between the highest ape and the lowest man. Certainly though, given his general outlook, given his emphasis on the socialistic need to keep up group fitness, the issue was of the highest importance for him. He was certainly a good candidate for conversion to Galton's secular religion of eugenics.

The foregoing, then, appears to comprise an important part of the course by which Pearson became the sort of social Darwinist socialist first depicted. . . . Let us turn now to the

epistemological component of his outlook - to the neo-Kantian instrumentalism which dictated the form of his scientific methodology, and to his belief that the path to virtue lay through the sort of science dictated by his epistemology. Once again, we return to Heidelberg, where Pearson studied under Fischer, though reading far more widely than Fischer's course demanded. By May 1879, he was reading Kant's Metaphysics of ethics, as a follow-up to the Critique of pure reason which he had meticulously studied whilst in Cambridge. Parker, interestingly, followed a parallel but lesser course⁷⁷

I suppose that I ought to read a lot of Kant, Fichte, Schelling and Hegel, but a sword hangs over my head in the shape of the tripos: I must feed on hope so far as reading is concerned, and meditate the while, what a lucky dog you are with your time at your disposal.

By May 25, Pearson was able to write to Parker, saying more about his work, and rejecting the possibility of a metaphysical foundation for ethical judgement.⁷⁸

You are certainly right about the foundation of religion not being the pure reason, this Kant I think has conclusively proved in the Kritik der reinen vernunft. In the Metaphysics of ethics and the Practical reason, he attempts to base a religion on morality, or a belief in God follows from the necessity of moral order in the Universe. This seems to me to be thoroughly unsatisfactory. He even contradicts himself by founding his moral system on a moral sense (conscience, which is innate and universal), which he asserts dogmatically to exist. Is this innate sense the same in the cannibal and the educated man? It is not empirical, according to Kant, and there is no question of its development. If then we can't found religion on morality we are left alone with the emotions, and the feeling of want, religiosity, and quite enough too.

Clearly, Kant was not his only interest, for, on June 12, Lawrence Green wrote, apologising that he was unable to give a suitable 'summary explanation' of Hegel. Parker was equally baffled by the German dialectician:⁷⁹

Green has told me that he heard from you the other day and that you had just embarked in the mysteries of Hegel: I hope that you have managed to understand him better than Balfour succeeded in making me do in his lectures.

By the 20th., Pearson's bafflement had worsened. He wrote to Parker, telling him of a dinner with Fisher at which he planned to tell his

mentor that philosophy was a vain pursuit, and that

I feel at a lower ebb of despair with regard to the truth than I have ever felt before in my life.

As to truth itself, it was a dubious affair.⁸⁰

Then let us consider whether it can be a law of nature. Does anyone know what we mean by this expression, the more I have studied science and physics, the more I see that we know nothing of what we call nature - of electricity, light and attraction we know nothing. What is the sense of calling light a vibration? Or that gravity is a force between particles of matter varying as the inverse square of the distance!!! The term was invented some hundred years ago to describe a phenomenon which it attempts to explain ... Besides, the whole tendency of modern philosophy since Kant is to assure us that the so-called laws of nature exist in our minds, are a logical necessity of our minds which impress them on the things in themselves for they can only observe things in such relations. Fancy truth a function of that absurd humbug man's mind!

In the following weeks, there was a great deal of discussion of Hegel and Kant in which the usual difficulties associated with the understanding of these authors featured strongly. On July 12, for example, Kant is said to be difficult on account of (i) the Ding an Sich, (ii), the 'want of a binding link between his various mind powers, such as pure and practical reason, sensibility and understanding', and (iii), the impossibility of a God whose 'sole existence proof or reason is morality'. Faced with such difficulties, Pearson decided temporarily ^{to} abandon philosophy; his faith in reason had been 'shattered by the purely negative results' which he has found in the works of the great philosophers. Fortunately, however,⁸¹

There yet remains Natural Science; what the whole world - philosophy cannot teach us through pure reason, perhaps nature can through experience. What is the good of philosophers discussing free will if they do not know how much of the disposition of the child depends on that of its parents; or the human soul, if man is merely a stage between the primeval 'Kaulquappe and that which we do not know? These are questions for science to answer. How long they will take to answer them is also unknowable. Centuries separated Gallileo and Newton, Newton and Darwin, and until they answered, it is best that philosophy should slumber.

His hope, he noted, was to go to Berlin to study with Helmholtz or Kirchhoff. But, by October, it had perished. He had decided to throw philosophy and physics to the winds, feeling incapable of making major contributions to either one. He would study law, and, henceforth, there

would be no more 'Hegel or Schelling', no more dabbling in 'integrals and disintegrals'. He would concentrate on a career at the bar.

Here, in the Heidelberg episode, we see again the roots of later developments; notably of a 'Kantian' philosophy of science emphasising the need to extirpate the Ding an sich from scientific discourse, in whatever guise it might appear. Laws of nature are already portrayed as 'a logical necessity of our minds'. The distaste for Kant's metaphysical approach to ethics was also important, and remained; so much so that, in 1883, when reviewing Fischer's Kritik der Kantischen Philosophie, we⁸² find Pearson speaking well of the critical philosophy, but ill of the Critique of practical reason. Thinking doubtlessly of the Oxford neo-Hegelians, he notes that there is,⁸³

an entire change of front, the door is to be thrown open to the whole body of emotionalists, mystics and metaphysical idealists.

Clearly, Pearson was open to a non-metaphysical account of ethics, or, at least, to one that seemed non-metaphysical. By judicious manipulation of Darwin's ideas he was able to come up with ethical and social philosophies which both satisfied the Spenglerian desires which, we have seen, he acquired in Germany and could be justified by reference to the findings of science and to the philosophy of science.

Thus were laid the outlines of Pearson's weltanschauung. The idea of reality as unknowable, and the idea of science as summariser of the phenomena were already there, and would be developed while preparing the text of Clifford's Commonsense of the exact sciences,⁸⁴ reading the works of Mach as he worked. The historicism, the naturalistic ethics and the political ideals would all be integrated within the language of social Darwinism. The eugenic state would appear particularly attractive against this backdrop. The passage to biometry and statistics linked with eugenics was a clear possibility.

The foregoing analysis of Pearson is a rather personal one, and might perhaps be augmented by a more broad-scale sociological analysis proceeding along the same general lines as Hobsbawm's explanation of the⁸⁵ emergence of the Fabians in late 19th century Britain. Pearson's thought, after all, which stressed the importance of the establishment of a socialist state governed by highly-trained professionals,⁸⁶ was similar in very general ways to the Fabian programme. Hobsbawm explains the emergence of the Fabians by referring to the changing nature of late 19th century British society - noting on the one hand the collapse of conditions which made old-style policies of economic laissez-faire seem a less attractive option than formerly, and, on the other, the emergence of a growing group of superior black coated 'workers by brain', occupying, as Hobsbawm puts it, a 'nouvelle couche sociale', the natural interests of which did not agree with those of the earlier 'entrepreneurial' middle class. These new men were, so to speak, the forerunners of Professor Galbraith's 'Technostructure', whose natural interest, Galbraith suggests, is to take over the running of the large corporations in a framework of elitist socialism. Hobsbawm, similarly, sees the style of professional socialism promoted by the Fabians as giving expression to the interests of the first British forerunners of the technostructure.⁸⁷

Pearson, qua salaried professional, had an identity of interest with this group, it might be thought, and may perhaps be seen as providing support for it in his thinking. Certainly, he provided a view of society in which the members of this group became depicted as of paramount significance, and, via his development of eugenic perspectives, he gave them a firm biological rationale for superior status. And, in his statistics, he might be seen as providing a suitable calculus for such an elite. In outline then, we can see that Pearson's response to the conditions of his life was one that reflected the interests of the social group with whom he identified. This, by turn, opens the possibility of explaining the overall pattern of his thought by reference to social structure and class interest. It is a possibility to which I shall return in the final chapter, which, *inter alia*, discusses the promise and the difficulty of such an explanatory tack.

Notes

Chapter 4

From the 'History of the biometric and Galton laboratories', published as an appendix to the University of London Senate minutes, 18 May 1920.

See E.S. Pearson, Karl Pearson: an appreciation of some aspects of his life and work, Cambridge (1938), 114.

A copy of this is kept in the Pearson archive, University College London.

For a conspectus of late 19th and early 20th century work in this area, see J. Arthur Thomson, Heredity, London (1908), also G. Archdall Reid, The principles of heredity, London (1905).

Marshall wrote on 'The graphic method of statistics' and Edgeworth on 'Methods of statistics'.

See e.g. Hobsbawm, Labouring men, London (1968). Also Paul Thompson, Socialists, liberals and labour, the struggle for London, 1885-1914, London (1967).

See also Bernard Semmel, Imperialism and social reform: English social thought, 1895-1914, London (1960). See chapter I, 'Social imperialism'.

For a good account of Stead and his work, see W.S. Smith, The London heretics 1870-1914, London (1967). The Bitter outcry was written by Andrew Mearns, secretary of the London Congregational Union.

William Booth (1829-1919) was the founder of the Salvation Army, and he was assisted by Stead in the writing of Darkest England.

See e.g. G. Stedman-Jones, Outcast London, Penguin books (1976). See chapter 16 'The threat of outcast London'. For a review of the social-investigatory literature of this period, see ed. P. Keating, Into unknown England, London (1976).

For details of these and other developments, see W.S. Smith, The London heretics, 1870-1914, London (1967).

P. Kropotkin, Mutual aid, London (1903). See also, G. Himmelfarb, Victorian minds, London (1968), see chapter 12, 'Varieties of social Darwinism

H.G. Wells, op.cit (see text), London (1902), 289-290.

S. Hynes, The Edwardian turn of mind, London (1968), 95.

4. Raymond Pearl, obituary of Karl Pearson, in Jnl.Am.Stat.Assoc., 31, (1936), 653-664.

15. A Stouffer, 'Karl Pearson - An appreciation on the 100th anniversary of his birth', Journal of the American Statistical Society, 53, (1958), 11-22.
 - . Cited in E.S. Pearson, op.cit (note 2), p. 47
 - . Karl Pearson, 'Mathematical contributions to the theory of evolution. 3. Regression, heredity and panmixia', Phil trans. Roy. Soc., 187A (1896), 253-318. See p.259.
 - . Ibid.
 - . See also chapter 6,
 - . C. Eisenhart, article on Karl Pearson in ed. C.C. Gillispie, Dictionary of scientific biography, New York, vol 10. (1974), 447-473. See p.451.
 - . For discussions of this, see E.S. Pearson op.cit (note 2).
 - . Notably in 'Mathematical contributions to the theory of evolution. 7: On the correlation of characters not quantitatively measurable', Phil.trans. Roy.Soc. 195A (1900), 1-47 and in 'Mathematical contribution to the theory of evolution. 8. On the inheritance of characters not capable of exact quantitative measurement', Phil.trans.Roy.Soc. 115A (1900), 79-150.
 - . W. Palin Elderton, Frequency curves and correlation, 2nd edn.London (1927).
 - . Pearson, op.cit. (note 22), 121.
 - . K. Pearson, 'Contributions to the mathematical theory of evolution,' Phil.trans.Roy.Soc., 185A (1894), 71-110.
 - . K. Pearson, 'Contributions to the mathematical theory of evolution. 2. Skew variation in homogeneous material', Phil.trans.Roy.Soc., 186A (1895), 343-414.
 - . See K. Pearson, 'On the criterion that a given system of deviations from the probable in the case of a correlated system of variables is such that it can be reasonably supposed to have arisen from random sampling', London, Edinburgh and Dublin Phil.Mag., 50 (1900), 157-175.
 - . i.e. for 1892, 1900 and 1911.
 - . See. C. Riddle, Karl Pearson's philosophy of science, unpublished Columbia Ph.D. thesis, (1958), 3.
 - . Karl Pearson, The Grammar of Science, London, (1892), 128.
 - . Ibid, 166.
32. See, for further details, chap. 5.

See note 16.

Ethel Elderton, 'The relative strength of nurture and nature', Eugenics Laboratory lecture series, (1909), 5-7.

L. Hogben, Statistical theory, London, (1957), 256.

For details of Thompson see R. D'Arcy Thompson, D'Arcy Wentworth Thompson. The scholar naturalist, 1860-1948, Oxford, (1958).

See chapter 3.

See G.M. Morant, A Bibliography of the statistical and other writings of Karl Pearson, London, (1939).

The text of this talk, entitled, 'The laws of inheritance according to Galton', is kept in the Pearson archive. See also chapter 6, note 4.

Pearson's copy of Natural inheritance is kept in the Pearson archive, University College London.

See Speeches at a dinner held in University College London, in honour of Professor Karl Pearson, privately printed, Cambridge (1934).

The intimate association between Popper's philosophical and social views may be seen by reading his intellectual autobiography, Unended quest, Fontana books, (1976).

Karl Pearson, The ethic of freethought, London (1888).

Karl Pearson, The Grammar of Science, London (1892), 34.

For Clifford's views, see W.K. Clifford, 'On the scientific basis of morals', (1875), reprinted in W.K. Clifford, Lectures and essays, 2 vols., London (1901), vol 2 74-95.

Pearson, op.cit (note 44), 425.

Ibid.

W.J. Hyde, 'The socialism of H.G. Wells in the early twentieth century', Jnl.hist.ideas, 17 (1956), 217-234.

Pearson, op.cit, (note 44) 435.

Karl Pearson, op.cit. (note 43), 324 (in his essay 'The moral basis of socialism').

Karl Pearson, 'Socialism and natural selection', The fortnightly review, 56 (1894), 1-21. This was republished in Pearson's Chances of death, 2 vols, London (1897).

Benjamin Kidd, Social evolution, London (1894).

Panmixia, a doctrine due to Weismann and others, held that, in the absence of selection, evolution would go into reverse.

Pearson, op.cit (note 51, 2nd referance) vol.I. pp 103-139. see p. 105.

[K. Pearson, Socialism in theory and practice , London 1884, reprinted in Pearson's Ethic of freethought, London. (1888). 346-369. See p 354.

Karl Pearson, National life from the standpoint of science, London, (1901).

Ibid, 43-44.

Karl Pearson, letter in Manchester Guardian, Feb 15, 1901.

See, for example, Pearson's essay on 'Reproductive selection' in his The chances of death, 2 vols., London (1897). See Vol I. pp. 63-102.

This, I think, is shown clearly in ch.7.

For a retrospective account, see K. Pearson, 'Old tripos days at Cambridge, as seen from another viewpoint', Mathematical gazette, 20, (1936), 27-36.

Pearson archive, University College London.

For a discussion of Marshall, see J.M. Keynes, Essays in biography, London (1951). For others, see F.M. Turner, Between science and religion the reaction to scientific naturalism in late Victorian England, London, (1974).

Pearson, op.cit. (note 62).

Pearson to Parker, Pearson Archive.

For an account, see J. Passmore, A hundred years of philosophy, Penguin books (1972). See chapters 2 and 14 in particular.

Pearson archive, University College London.

G.H. Quincke (1834-1924) - who pioneered practical physics tuition.

K. Pearson (under pen-name 'Loki') The New Werther, London (1880).

Ibid., 33.

71. Ibid, 33-34.
72. K. Pearson, 'Political economy for the proletariat. Introductory', The Cambridge review, 3 (1881), 123-126.
73. Ibid.
74. For a discussion, see Semmel, op.cit. (note 6), 203 et.seq.
75. Cited in R.D. O'Butler, The roots of national socialism, 1783-1933, London (1941), 247. In this context, see also M. Malia, Alexander Herzen, London (1961). See chapter 5, 'Schelling and idealism'.
76. K. Pearson, 'The woman's question', reprinted in the Ethic of free-thought, op .cit. (note 43), 371.
77. Parker to Pearson, 7 May 1879. (Pearson archive).
78. Pearson, to Parker: 25 May 1879. (Pearson archive).
79. Parker to Pearson. (Pearson Archive)
80. Pearson to Parker, 20 June 1879 (Pearson archive).
81. Pearson to Parker, July 12 1879 (Pearson archive).
82. K. Pearson, 'Kuno Fischer's new critique of Kant', Cambridge review 5 (1883) 109-111.
83. Ibid.
84. See W.K. Clifford, The commonsense of the exact sciences 5th edn. London (1907). In the preface, Pearson wrote of the difficulty of expanding the ideas of Clifford, noting that (ix) 'I should hardly have ventured to put forward these views had I not recently discovered that they have (allowing for certain minor differences) the weighty authority of Professor Mach, of Prag.' The preface is dated February 26, 1885.
85. See E. Hobsbawm, Labouring men: studies in the history of labour, London (1968). See chapter 14, The Fabians reconsidered.
86. See his views cited in chapters 4 and 7.
87. J.K. Galbraith, The new industrial state, 2nd edn. Penguin books (1972). See ch 6. 'The techno structure'.

Chapter 5. The Mendelian-biometrician controversy: problems in the history of genetics.

Introduction

So far, the biological side of biometry has hardly surfaced, as I have mentioned it mostly in its role as the midwife of statistics. But, it did have a very real and strong biological aspect, and it is not too much to claim that in constructing their new mathematical and biological discipline, Pearson and Weldon saw themselves not as adding to an existing repertoire of approaches to the central problems of evolutionary biology in the broad sense, but rather as displacing them - as replacing them with a more properly scientific alternative. Everywhere in the biometric literature we find references to the creation of a new style of biologist, who would be capable of deploying the new mathematical tools of the biometricians' trade. Weldon, in particular, was an active crusader amongst biologists, and, shortly before the formation of Biometrika, wrote to Pearson that:¹

The contention that 'numbers mean nothing and do not exist in nature' is a very serious thing, which will have to be fought. Most other people have got beyond it, but most biologists have not.

There can be no doubt that Pearson and Weldon, in the pages of Biometrika and within the walls of the biometric laboratory, saw themselves as starting a new biological movement. They were striving to change the course of British biology and to install themselves as helmsmen.

Clearly, they did not succeed in their ambition, and a central reason for their failure was their defeat in a long and bruising encounter with another school of British biologists - the Mendelian school, which gathered strength about the central figure of William Bateson in Cambridge in the early years of the 20th century. Bateson's 'victory' was not total, and the biometricians did not disappear without exerting some influence in the area of evolutionary studies. What this influence was we shall shortly see, as this chapter is devoted to a study of the famous 'Biometric-Mendelian' debate which was such a notable feature of early 20th century British biology. It was a debate in which the Mendelians advocated and sought to establish the new Mendelian genetics as biological orthodoxy, and in which the biometricians bitterly opposed Mendelism.

The debate has at least three sorts of interest. There is, firstly, the fascination of the arguments deployed and of the theories advocated. Then there is the historiography² which the debate has generated: different historians have offered different and inconsistent

explanations of a set of 'historical facts' about which they seem largely agreed. Finally, there is the issue of the influence which the biometricians managed to exert on the development of biology despite their defeat in the encounter with the advocates of Mendelian genetics.

In the remainder of the chapter these topics.

find a place. Thoughts on the third are scattered throughout the chapter and are brought to a head later in the work - after we have seen more of the work of R.A. Fisher.

The chapter has three components, (i), a 'standard' chronological account of the debate; (ii), a discussion of rival historical interpretations, and (iii), an attempt at arbitration and synthesis. No apologies are offered for introducing historiographical issues, for historical analysis is not chronology and 'the facts' do not speak for themselves. Finding the best interpretation or explanation of the facts is the business that makes history a thrilling theoretical enterprise.

The debate

(A) The facts of the case

We may commence with the career of the father of English Mendelism - William Bateson (1861-1926).³ He was the son of the master of St. John's College Cambridge, who, by turn, was the son of a Liverpool merchant. William was extremely unhappy at school, but, on entering Cambridge found a new life in the study of biology under the 'new wave' of biologists whose appearance had been ushered in by the appointment of Michael Foster to the Trinity praelectorship of physiology in 1870. Bateson did well, and graduated with a first class degree in 1882, specialising in zoology. Thereafter, he went on to do research, following the dominant Cambridge pattern of phylogenetic morphology which was then so heavily promoted by men like Adam Sedgwick and F.M. Balfour.⁴ In his work, Bateson sought to throw light upon the origins of the vertebrates⁵ through a study of Balanoglossus, a worm like creature showing a notochord in its proboscis which has a larval form that is free swimming, ciliated and shows resemblances to the Echinoderms. This animal, and allies, Bateson placed in a special class which he called 'Hemichorda'. Balanoglossus he hoped, might 'betray the secret of chordate and hence vertebrate origins'. He produced work that was very well received when published in 1884, 1885 and 1886, but which also telegraphed to the reader

a growing conviction on Bateson's part that his discipline had the most insecure of foundations.⁶ Interestingly, at the time when he was pursuing these early studies in Cambridge, Bateson was close to the man who was subsequently to become his greatest adversary - namely W.F.R. Weldon, whom I have already discussed in some detail. At the time of Weldon's death, Bateson wrote in letters ^{to W.F.R.} of his bitter-sweet feelings for Weldon.⁷

I owe a great deal to him. It was through the chance of meeting him that I first became a zoologist, and afterwards through him that I got my first start with Balanoglossus.

"Until the time - about 16 years ago - when his mind began to embitter itself against me, I was more intimate with him than I have ever been with any one but you.

"...If any man ever set himself to destroy another man's work, that he did to me - and now suddenly to have one of the chief preoccupations of one's mind withdrawn, leaves one rather "in irons", as sailors say'.

And,

"To Weldon I owe the chief awakening of my life. It was through him that I first learnt that there was work in the world which I could do. Failure and uselessness had been my accepted destiny before.

"Such a debt is perhaps the greatest that one man can feel towards another; nor have I been backward in owning it. But this is the personal, private obligation of my own soul'.

By 1886, plainly, Bateson was thinking about the need for a new style of evolutionary biology - one disciplined by a 'fuller understanding of the laws of growth and variation'. And, his work for the next few years took him into just this area, climaxing in 1894⁸ in the publication of his Materials for the study of variation. His rejection of the traditional seems to have cost him dear. Weldon's job did not go to him when Weldon left for the chair of zoology at University College in 1890, and Bateson heard that this was because he had taken up a 'fancy subject' - i.e., the study of the very basic processes of variation which, he considered, would have to be fully understood before discussions of evolution could be sensibly pursued.⁹ Bateson was left with just his fellowship, supplemented in 1892 when his college, St. John's, appointed him steward.

The publication of the Materials was a major event. For the most part it catalogued variations seen in living organisms, but its introduction and conclusion packed a particular sting. Here, Bateson lambasted traditional Darwinians, attacking on two fronts. On the one hand,¹⁰ he *attacked* their use of embryology, arguing that it had 'provided us with a magnificent body of facts, but the interpretation of the facts is still to see', and that, as things then were, any one engaged in Darwinian phylogeny was 'at liberty to postulate the occurrence of variations on any lines which may suggest themselves to him, a liberty which of late has been freely used.' On the other, he accused them of having failed to establish their 'utilitarian view of the building up of species' - i.e., the view that each feature of an organism could be construed as advantageous to its bearer.¹¹ And he now advanced the view that the differences between species were abrupt and discontinuous, and that, for a variety of reasons, it made sense to suppose these due to discontinuous variations - a view directly opposed to that of the Darwinians, who saw the differences between species as the product of the long selection of the small variations shown by all the members of large populations. Bateson proposed another view. The traditional view, he said, had overestimated the plasticity of the organism - seeing it as putting out small random variations in all directions, which natural selection would assess for utility, thus pressing the organism into close adaptation to its environment. Bateson, by contrast, opposed high plasticity, and emphasised the power and the autonomy of the process of variation, which he now saw as capable of yielding, at a stroke, new organisms discontinuously related to existing forms, but still well-functioning. For him, it was not necessary to suppose every feature of an organism present on account of its utility: for, when a new form was produced discontinuously, it would stand or fall as an entity. Natural selection would ban it or let it pass.¹² If we were to take a single example of the sort of thing that Bateson had in mind, we might consider the tulip with all its parts in fours, arising as a variation of parent tulips, whose parts were present in triplicate. The variation, he noted, was complete and perfect.

Here, I have merely scratched the surface of Bateson's thought on variation. It is worth noting that he supported his views in two ways. He had ^{firstly} a set of arguments for the impossibility of continuity in evolution, resembling in part the remarks of another opponent of selection, T.H. Huxley, who, before the Origin, had put matters nicely when he asked rhetorically,¹³

who has ever dreamed of finding an utilitarian purpose in the forms and colours of flowers, the sculpture of pollen grains and in the varied figures of the frond of ferns.

This level of ostensive argument was underpinned by a rich though vague theoretical orientation.¹⁴ Stemming from his early period, when he had to consider the evolutionary origin of metameric segmentation in animals, Bateson had been much impressed by a number of ideas - by the idea of symmetry in organisms, by the idea of repetition of parts, by the idea of variations in these being due to 'definite changes in the mechanical relations of dividing parts', and by the idea of resemblance between the patterns exhibited by organisms and those due to natural, mechanical and rhythmical processes. All of this went hand in hand with a vibratory theory of inheritance, based on a biological analogue of the vortex theory of the atom, which was to lead Bateson into long non-acceptance of the chromosome theory. The general style of his thought at this level is caught in the following extract from his Problems of genetics. This work was published in the 20th century, but work by Coleman shows that the style of thought which it embodied dated from a much earlier period.¹⁵

Attempts have lately been made to apply mathematical treatment to problems of biology. It has sometimes seemed to me that it is in the geometrical phenomena of life that the most hopeful field for the introduction of mathematics will be found. If anyone will compare one of our animal patterns, say that of a zebra's hide, with patterns known to be of purely mechanical production he will need no argument to convince him that there must be an essential similarity between the processes by which the two kinds of patterns were made and that parts at least of the analysis applicable to the mechanical patterns are applicable to the zebra stripes also. Patterns mechanically produced are of many and very diverse kinds. One of the most familiar examples, and one presenting some especially striking analogies to organic patterns, is that provided by the ripples of a mackerel sky, or those made in a flat sandy beach by the wind or the ebbing tide. With a little search we can find among the ripple-marks, and in other patterns produced by simple physical means, the closest parallels to all the phenomena of striping as we see them in our animals. The forking of the stripes, the differentiation of two 'faces,' the deflections round the limbs and so forth, which in the body

we know to be phenomena of division, are common both to the mechanical and the animal patterns. We cannot tell what in the zebra corresponds to the wind or the flow of the current, but we can perceive that in the distribution of the pigments, that is to say, of the chromogen-substances or of the ferments which act upon them, a rhythmical disturbance has been set up which has produced the pattern we see; and I think we are entitled to the inference that in the formation of patterns in animals and plants mechanical forces are operating which ought to be, and will prove to be, capable of mathematical analysis. The comparison between the striping of a living organism and the sand-ripples will serve us yet a little farther, for a pattern may either be formed by actual cell-division, and the distribution of differentiation coincidentally determined, or - as visibly in the pigmentation of many animal and plant tissues - the pattern may be laid down and the pigment (for example) distributed through a tissue across or independently of the cell-division of the tissue. Our tissues therefore are like a beach composed of sands of different kinds, and different kinds of sands may show distinct and interpenetrating ripples. When the essential analogy between these various classes of phenomena is perceived, no one will be astonished at, or reluctant to admit, the reality of discontinuity in Variation, and if we are as far as ever from knowing the actual causation of pattern we ought not to feel surprised that it may arise suddenly or be suddenly modified in descent.

All of this must have been unsympathetic to Weldon, who, we have seen, had moved from Cambridge to London in 1890, abandoning also the Cambridge phylogenetic approach to evolution in favour of a new statistical approach based on Galton's mathematical techniques. But though Weldon abandoned the Cambridge style of research, he could have enjoyed but little fellow feeling for Bateson, who had also defected - but, so to speak, to a different country. For, as we have seen, Weldon was a strict Darwinian, in the sense that he believed evolution to proceed via the action of natural selection upon the small continuous variations displayed by all the members of large intercrossing populations. It was this sort of Darwinism that Bateson opposed, and, as may be surmised from the quotation above, he was not more favourably disposed to Weldon's research¹⁶ style. This, in best Pearsonian manner, stressed the concentration upon appearances. Bateson, by contrast, wanted to understand the underlying biological processes by which variations were produced. He never got very far in his quest, but we can at least see that it really was his quest, and that it could lead him into making comparisons between biological

processes and such physical processes as the passage of a vortex down a stream or the patterns on Chladni plates. So, if we look at Weldon and Bateson in the early 90's, we see that each had rejected aspects of traditional morphology and that each had gone on to study the process of evolution directly: but the style of study and the views on the process which they were developing were incompatible. One further example of the difference in their thinking is given by the use which they made of the biological notions of adaptation and fitness. Bateson was in the habit of arguing that, in many cases, the differences between species were such as to make it implausible to suppose that the species in question represented the termination of a series of forms, each merging imperceptibly into its predecessor and successor and with each one having a slightly better degree of adaptation than its predecessor.¹⁷ The argument was always poorly put, and was frequently unclear, but it shows Bateson thinking in terms of fitness and adaptation. These, interestingly and significantly, were just the concepts which Weldon was anxious to eliminate from the new biometrical biology¹⁸

Knowing that a given deviation from the mean character is associated with a greater or less percentage death rate in the animals possessing it, the importance of such a deviation can be estimated without the necessity of inquiring how that increase or decrease in death rate is brought about, so that all ideas of 'functional adaptation' become unnecessary.

Weldon, of course, was not alone, for he had recruited Karl Pearson to his cause, and, together, they were working upon the development of the research programme outlined in Weldon's manifesto reproduced on p. 8. We shall see more examples of the style of work that the biometricians were engaged upon as the account progresses, but, for the moment, it is good to note that the approach to heredity which they favoured was one that was essentially phenotypic, and which concentrated upon the establishment of laws governing the inheritance of continuously varying characters. The 'ideal type' of law which Pearson and Weldon sought, and which they were wont to refer to as the 'law of ancestral heredity', for example, was one of the following sort

$$x_0 = a_1x_1 + a_2x_2 + a_3x_3 + \dots$$

where, as E.S. Pearson has put it,¹⁹ 'x₀ is the expected deviation for the offspring from the mean of this generation, x₁ is a linear function of the

deviations of the two parents, x_2 a similar expression for the four grandparents and so on.' The central problem, to be discussed in the following chapter, on the 'law of ancestral heredity', was to find the proper values for the coefficients a_1 , a_2 etc. in different cases - e.g., height in man, coat colour in horses and so on.

A clash with Bateson, who saw the proper unit of evolutionary advance as the discontinuously varying individual, was, unsurprisingly, not long in coming. Hostilities opened in 1894 when Weldon reviewed Materials²⁰ unfavourably in Nature, discounting Bateson's arguments for discontinuous variation. Bateson, Weldon considered, was in danger of confusing genuinely discontinuous variations with variations that found a proper place in the 'tails' of continuous frequency distributions. In 1895 a similar argument was pursued when Bateson and Weldon²¹ found themselves on opposite sides in a controversy concerning the origins of the cultivated Cineraria. Thiselton Dyer had argued that it was the product of selection. Bateson denied this, arguing that it was the consequent of hybridisation and sporting. Weldon entered the fray on Dyer's side, and accused Bateson of 'want of care in consulting and quoting the authorities referred to'. Thereafter came an acrimonious exchange of views²² concerning the paper of 1894 in which Weldon, under the imprimatur of the Royal Society 'Committee for conducting statistical inquiries into the measurable characteristics of plants and animals' claimed to have demonstrated the existence of a selective death rate amongst the continuous variations in 'frontal breadth' shown by Plymouth crabs.²³ Bateson was anxious to show that Weldon had not given a genuine example of Darwinian evolution in operation, and wrote copious letters to Galton, chairman of the committee. In consequence, Galton made room for Bateson on the committee, along with several other traditional biologists. Naturally, the new, enlarged committee was entirely split, containing as²⁴ it did, dynamic advocates with very different ideas about what carrying through its purposes actually entailed. In the event, Pearson and Weldon resigned in 1900, and their resignations were followed by Galton's. This left the committee in the hands of its most dynamic member - namely William Bateson, whose relations with the biometricians took a further turn for the worse in 1900 when Bateson prepared a paper critical of²⁵ Pearson's work on 'homotyposis' (to which we shall return), which was

printed before the object of its criticism had been published by the Royal Society. Pearson responded with a paper 'On the fundamental conceptions of biology', which he published in the newly-formed Biometrika.²⁶ Bateson offered a response, but Pearson would not allow it into Biometrika unless it was reduced to 6 pages or less, and, in consequence, Bateson circulated privately a privately printed paper on 'Variation and differentiation in parts and brethren', which he caused to be set up after the manner of a Biometrika article.²⁷

1900, of course, was also the date of the rediscovery of Mendel's ideas by de Vries, Correns and Tschermak²⁸ and Bateson, who had been thinking along lines which were similar to those being followed by De Vries, was able to see a great future in the new Mendelian genetics. He embraced it fervently, particularly as it offered a picture of unit characters, integrally inherited, which jibed nicely with his views on variation and evolution. Within the Mendelian framework he could see a way of explaining, via mutation, the creation of new, discontinuous variations, and via the doctrine of the purity of the gamete, the non-blending inheritance of those variations. He gathered a group of workers about himself which contained, at various times R.C. Punnett²⁹ (future Arthur Balfour professor of genetics at Cambridge University), Miss E.R. Saunders, a botany lecturer at Newnham College, Captain C.C. Hurst (author of Experiments in genetics, 1925), Leonard Doncaster (author of Heredity and noted cytologist), R.P. Gregory (who died at a tragically early age), R.H. Lock (author of Recent progress in the study of variation, heredity and evolution), Miss Sollas, Miss Marryatt and others. The work of the Bateson group was published, at first in the Reports³⁰ of the evolution committee, and later, in the Journal of genetics, founded by Bateson and Punnett in 1910.

Thus, by 1902, the two groups had their own bases - London and Cambridge their own platforms - Biometrika and the Reports, and were gathering followers. Now the debate entered its hottest phase. Weldon, in Biometrika, attacked the Mendelians' work in strong language, accusing the Mendelians of making ad hoc adjustments to their theories,³¹ of falsely claiming that heterogeneous groups - e.g. round or wrinkled peas - could be regarded as homogeneous, and of maintaining the principle of the purity

of the gamete in the light of refuting evidence. The tenor of the conflict was so fierce that, at the British Association meeting for 1904, the well-founded expectation of a blazing row between Weldon and the Mendelians attracted more than a full house.³² Those who sought a gladiatorial spectacle were, it seems, not disappointed - even though they frequently had to sit on the window ledges. Weldon was able to make some good points against the Mendelians, though he lost face when his student A.D. Darbishire recanted his opposition to Mendelism and went over to the enemy.³³ (We have seen that Pearson was keen for Darbishire not to work in Galton's laboratory.) In one celebrated incident, Weldon was able to discredit some work of Hurst's by reference to the breeding records in the General studbook, but Hurst hit back with a reference to Form at a glance, which indicated the spuriousness of the supposed counter-example.³⁴

I have not entered into details of the various arguments, as these have been nicely recorded by W. Provine in his recent work on The Origins of theoretical population genetics, and because the arguments rather lose their interest when one acknowledges, as the late Imre Lakatos so brilliantly pointed out, that the structure of scientific theorising and of inter-theoretic debate is not such as to allow for any instant refutations along the lines which Popper in some of his incarnations seems to suggest.³⁵ But, it is well worth mentioning, the Bateson group had a number of explanatory successes (though the biometricians tended to see these as consequent upon the introduction of opportunistic assumptions into genetics), notably in the area of epistasis, the recognition of intermediate dominance and of linkage (which the Bateson group 'saw' in terms of 'coupling' and 'repulsion').³⁶ Bateson, interestingly, was a firm opponent of the chromosome theory (i.e., the theory that Mendelian factors were physically localised in the chromosomes), and would not grant formal recognition to the chromosome theory until the twenties - that is to say, until ten or more years after the emergence of the dynamic Morgan school of geneticists in the U.S.A.³⁷

Similarly, I have not bothered to place Bateson's work against the labours of foreign geneticists, not for chauvinistic reasons, but because this has already been done in other works. But, it is useful to note³⁸ that the period 1900 to 1920 saw such work as (a) De Vries' 'mutation theory' (b) Johannsen's work on 'pure lines', and (c) the emergence of the 'Morgan school' in America and its developments of cytogenetics to the

point where chromosome mapping - i.e. the practice of assigning a particular chromosomal location to a particular factor - became feasible. A very readable account of these developments may be found in the recent publication of the Open University on the history and social relations of genetics.

Weldon died in 1906. Before Weldon's death Pearson had not reacted to the new genetics in quite so hostile a manner as Weldon. But he had been generally unreceptive to Bateson's ideas on evolution before 1900, and, after 1900, he was unreceptive to the new genetics. He did not attack the Mendelians with the same force as Weldon had done - except when Mendelians attempted to invade his favoured preserve of eugenics, as, for example, when the American eugenicist Davenport claimed that mental defect was a Mendelian recessive and was pilloried by the astute Pearson for his pains.³⁹ All in all, he showed a continuing preference for the study of heredity via his own statistical techniques, which he saw as having a broader applicability than Mendelian theory and as 'having nothing whatever to do with any physiological hypothesis.'

After Weldon's death, Pearson gave more time than previously to eugenics, and when he was able to set up his Department of Applied Statistics in 1911, kept Mendelians out of the department. He also seems to have been responsible for the removal of Davenport and of Raymond Pearl from editorial positions on Biometrika on account of their Mendelian sympathies.⁴ Naturally, therefore, Pearson kept Mendelian genetics out of the pages of Biometrika - even though this was a journal for the 'statistical study of biological problems' and, as we now realise, Mendelian genetics was a subject eminently suited to the incorporation of statistical methods. An important consequence of his distaste^S for Mendelism, therefore, was that, in Britain at least, the leading institutional repository of statistical expertise was kept separate from Mendelian genetics. Nowadays, of course, the mathematical study of heredity and evolution is the mathematical study of Mendelian genetics and of the evolution of Mendelian populations. This new style of work, in Britain at least, had to arise from the labours of men who were not Pearson's uncritical disciples - men like J.B.S. Haldane and R.A. Fisher who were familiar with Pearson's work without being committed to it, and who were able to establish themselves institutionally so as to be free from Pearson's domination. Ironically, Fisher was to succeed Pearson in the Calton chair on his retirement in

1930, and J.B.S. Haldane was the first incumbent of the Weldon chair of biometry. So, in the long run, the desire of the founder biometricians for a mathematical science of heredity and evolution which did not encompass Mendelism was frustrated even within the institution where the desire had been nurtured.

So far, I have given very little by way of precise illustration of the arguments deployed in the biometric-Mendelian debate, other than to summarise Weldon's objections and to mention, implicitly perhaps, Bateson's hope that, given experiments enough and time, the Mendelian explanatory schematism could be extended so as to encompass all the phenomena of heredity. This is because other historians, notably Provine, have recorded the events with precision and skill. But, there is room for a couple of vignettes which express something of the different aspects of the debate. The first concerns simply a paper contained in the short-lived Mendel journal, written by 'Ardent Mendelian' - possibly Bateson himself - whose general tenor and imagery tells us something of the passion aroused in the debate. The second concerns Pearson's own investigations of Mendelism and his rather amazing rejection of his own results. Both episodes tell us a good deal about the nature of the debate in Britain.

(a) 'Ardent Mendelian'

Here, amidst the fine phrases, we can see something of the Mendelians' hopes of extending their analyses so as to include the continuous variation studied by the biometricians. The polemic is written with a verve and wit that constantly eluded their opponents.⁴¹

There exists a Guild of very active and strenuous students which is known to science and to others as the Biometrical School. Its devotees and exponents are noted for the number and diversity of their pilgrimages and expositions, for they are prepared to apply mathematical methods to any problem, ranging from the infinitely little in the realms of Biology and Pathology to the infinitely great in the stellar domains of Astronomy.

It is true that when at last, after a weary journey over thorny paths, they reach the temple of their respective pilgrimages, the reception extended to them is not always gracious. For the gods of Anatomy, Biology, Medicine, Astronomy, and, we regret to say, even some of those of Mathematics, do not always anoint the pilgrims with unctuous and fragrant ointments, for too often that which is expected by them to be balm is rendered escharotic by the gods.

With regard to the organisation of this Guild we are led to infer, on the analogy of the maxim of "your corn in my bushel," that the Biometrical School is organised on the lines of a field army, and that its constitution comprises at least a supreme "field-marshal," a "staff-corps," and a "rank and file." We believe at the mobilisation of this scientific army martial law was proclaimed, and that it has not yet been reclaimed. We may further infer, therefore, that the discipline of the army is very severe, and perhaps this may throw some light upon the constant reappearance of the figure 0.5 in relation to the size of some of its artillery equipment. We believe further, from certain information which the dispatches of the "Field-marshal" reveal, that the army has also its ambulance corps, consisting of "higher-consultants" and "general practitioners." We have not the slightest doubt that such a militant organisation has urgent need for an ambulance branch, and that its duties must be incessant. And, when we review the many battles with the gods in which it has been engaged, and we recall their disastrous results, we find an explanation of the anomaly, that whereas other armies are content with "general practitioners", the biometrical one finds it necessary to retain "higher consultants".

In some respects it is a very fine army, and it is certainly an imposing one upon parade. It is led, officered, and manned by men of transcendent intellect, of whom any country may be proud. It is an army which in some domains may have achieved some eminent victories for truth: but in other domains we are afraid our judgement compels us to say it has but obscured the topography and geography of the country of its invasion by the smoke of battle, produced by the burning of its "correlation" gunpowder, and that it has failed to capture the Temple of Truth by the errors of its strategy and the ineffectiveness of some of its weapons of attack.

Opposed to the Biometrical army is the Mendelian. More recent in origin, less martial in organisation, but very vigorous, the Mendelian army has already turned the flanks and pierced the centre of the older one opposed to it. For signs of surrender on one wing, and of retreat, very skilfully covered, on the other, are visible in the biometrical ranks. The broken centre, encouraged by the boldness and coolness of its eminent Field-Marshal - who like the kings of old personally fights on the battle-field - is making a rally on the high grounds to the rear. These hills are marked on the Mendelian map as very rugged and difficult of ascent, not to be rushed by brilliant cavalry charges, but not impregnable before the persistent slow, and methodical onslaught of a courageous and patient infantry; they are named the hills of "Masked Segregation". On the biometrical map they are marked as impregnable, when once occupied and entranced, and are named "Continuous or Fluctuating Variations", or, in their more recent maps, as "Intermediates".

165

The great battle of the future is that which will be fought along this rugged range of the "Intermediates". The task of the Mendelian army is to take it. And, already in the plains below its brigades are beginning to deploy, and are making those initial dispositions which indicate that the assault is being preapred. At the same time, far away on the enemy's flank, in the valleys of Copenhagen, a great turning movement is being developed, and the brigades of the "pure lines" are preparing for their march along the dip-slope of the range, in order to strike the Biometrical army in its rear at the moment when the main Mendelian army unfolds its frontal attack up the rugged face of the escarpment.

(b) Pearson and continuous variation

Had 'Ardent Mendelian' but known it, the problem of giving a Mendelian account of continuous variation was well under its way to solution at the hands of the biometricians themselves. In what follows, I will discuss Pearson's work in that field, as it illustrates very nicely the extent of the biometric disdain for Mendelian genetics. It helps one to see the extent of an historical problem whose resolution may offer useful insights into the sorts of considerations that can enter into the minds of leading scientists when evaluating a new theory.

We may start by briefly recalling the problem of continuous variation. Pearson and Weldon, and indeed Galton too, had found that populations frequently offered a Gaussian distribution when considered in respect of some suitable dimension - e.g., human stature. They had also shown that fathers and sons, brothers and brothers, nephews and uncles and so on, were connected by a series of correlation coefficients - a finding, we have seen, which was made first by Galton. Pearson had estimated the correlation between father and son in respect of human stature to be about 0.5.⁴² An obvious problem for anyone wishing to advance the claims of Mendelian genetics to be a universally applicable scheme of genetics was the problem of showing that it could both account for the existence of normal distributions of variation in populations and for the observed correlations linking relatives.

Pearson addressed this issue in a paper published in 1904.⁴³ Using simple combinatorial algebra he explored the consequences of the following Mendelian model. He supposed that, say, height, depended upon the state of n Mendelian loci. At each locus three genotypes were possible, namely AA, Aa and aa. He supposed dominance to be complete,

so that if we regarded each locus as determining one or no units of stature, the contributions would have been as follows

AA or Aa ——— 1 unit
aa ——— no units.

By supposing that, in the population as a whole, the relative frequencies of the two alleles A and a were both equal to $\frac{1}{2}$ each at every locus, he was able to show that the model, under a supposition of random mating, led to expectations of (i) a normal distribution of stature, whose mean and variance remained unchanged from one generation to the next in the absence of selection, and (ii) linear correlation between relatives, with the father-son correlation in particular, taking the value $1/3$. The value of n was found to be irrelevant, so far as the correlation coefficient was concerned. In practice, as noted, the observed value was closer to 0.5., and, moreover, the value of the coefficient had been found to vary from organ to organ, and from species to species.

Clearly, there were two ways in which such a result could be interpreted. One might say that it was confirmatory of Mendelism because the main qualitative features of the situation had been explained - and on the basis of a particularly simple model at that. On the other hand, one might stress the difference between observed and predicted values for the correlation coefficients connecting relatives of different degrees, and argue that the discrepancy added up to a refutation of Mendelism. Pearson unswervingly took the latter course, thereby laying the foundations for what was to be a long lasting belief that Mendelism and continuous variation were incompatible. He was very severe.⁴⁴

Unfortunately, even such a general pure gamete theory as we have here dealt with, while leading to results which form a special case of the law of ancestral heredity, is not sufficiently elastic to cover the facts. The lesson to be learnt from the present investigation is, however, that there is no essential repugnance between any of the main results of the biometric school and a theory of the pure gamete, but on the contrary, it is perfectly possible to test such theories by biometric methods. We may fairly ask anyone who propounds in future a Mendelian or pure gamete formula as a theory of heredity to remember that it involves in itself definite laws regulating the reproduction of a population mating at random, and that it is incumbent upon the proposer to test whether or not such laws are consistent with what we already know of the inheritance statistics of such populations. When we remember that deducing all the effects of such a formula within the whole field of inheritance will almost always form a very laborious piece of mathematical analysis, there seems a touch of scientific

irresponsibility in propounding an immense variety of formulae to suit one or other special case, and then modifying or withdrawing them when they are found to fail in another.

Fortunately, however, Pearson's associate G.U. Yule was able to see⁴⁵ that Pearson's model was a rather restricted one, and, in a paper delivered in 1906, pointed out that were Pearson to relax the two assumptions (a) that the relative frequencies of A and a factors were $\frac{1}{2}$ and $\frac{1}{2}$ at each locus, and (b), that dominance was total, then, indeed, observed correlation values could after all be accounted for within the Mendelian scheme.

Pearson, by now already at odds with Yule over Yule's criticisms of other sections of his work, did not respond immediately, but did return to the matter in 1909, writing in the⁴⁶ first instance against a paper written by Weldon's former assistant A.D. Darbishire, who, as noted, had defected to the Mendelian camp. And, in one of the papers written in that context, written, it should be noted, after Hardy had proved the "Hardy-Weinberg law", Pearson took up some of Yule's suggestions, and was able to derive results supplementing those of the 1904 paper. He showed in particular, that in the case of total dominance, the expected correlation between son and parent would be equal to $(q/(1+q))$, where q is the relative frequency of the recessive allele a . In the case that we take $q = \frac{1}{2}$, we have the expected correlation taking the value $1/3$, which is the value gotten in the 1904 paper, though the range of values obtainable by allowing q to range from 0 to 1 stretches from 0 to $\frac{1}{2}$. Pearson also showed that, in the case that there was no dominance, i.e. when the phenotypic value of Aa was intermediate between that of AA and that of aa , then the expected value for parent offspring correlation was $\frac{1}{2}$, a value which was independent of the value of q .

One might have expected Pearson to have responded to his new results by acknowledging that his earlier objections to Mendelism had been overcome. But he did not. Instead, he made a point of denying that Mendelian genetics allowed for intermediate dominance, and suggested that this point prevented the reconciliation of Mendelian theory with observation.⁴⁷

There is, however, I venture to think, another aspect of these results which is worthy of fuller consideration. Namely, the fairly close accordance now shown for the first time to exist between the ancestral gametic correlations in a Mendelian population and the observed ancestral somatic correlations shows that the accordance between gametic and somatic correlations is for at least certain characters possibly more intimate than is expressed by the absolute law of dominance. If (Aa) were a class, or possibly on a wider determinantal theory a group of several classes marked by an individual somatic character - not invariably identical with the somatic character of (A.A.) - there would be little left of the contradiction between biometric and Mendelian results as judged by populations sensibly mating at random. It is the unqualified assertion of the principle of dominance which appears at present as the stumbling block.

His assessment, therefore, appears to be one of a man held back from accepting Mendelism because the Mendelians continued to make an 'unqualified assertion of the principle of dominance', thereby ensuring that the high values for gametic correlation could not be interpreted as phenotypic correlations. This, one might think, was the reason why Pearson held back and allowed the title of synthesiser of biometry and Mendelism to go to R.A. Fisher, who offered a comprehensive account of the relations between Mendelian genetics and continuous variation in his famous paper of 1918 'On the correlation between relatives on the supposition of Mendelism inheritance'. But, this seems improbable, for, as early on as 1902, in reply to one of Weldon's Biometrika articles, Bateson, in his Mendel's principles of heredity: A defence, a work well known to Yule and Pearson, made a great point of denying Weldon's allegation that Mendelism involved a 'law of dominance'.⁴⁸

The whole question of whether one or other character of the antagonistic pair is dominant, though of great importance, is a logically subordinate one. It depends on the specific nature of the varieties and individuals used, sometimes probably on the influence of the external conditions and on other factors we cannot here discuss. There is as yet no universal law here perceived or declared.

In 1909, in his Mendel's principles of heredity, Bateson reinforced the point, pointing to 'many cases where dominance is imperfect', cases, he felt, that were quite consistent with his favoured 'presence and absence' theory of gene-action (or, more precisely and less anachronistically, Mendelian factor-action).⁴⁹

In cases where the pure dominants are recognizably distinct from the heterozygous dominants, it must naturally be supposed that two 'doses' of the active factor are required, one from the paternal, and another from the maternal side, in order to

We can be quite certain that Pearson had heard of this stance, as there is extant a letter to him from Weldon, dated August 11, 1903,⁵⁰ in which Weldon asks Pearson to consider 'Bateson's suggestion that in cases of blending inheritance the units are many and dominance is "absent indefinite or suppressed" (Defence - p.115)'.

All of this suggests that the matter of dominance was, so to speak, merely a front, an objection under which Pearson could publicly shelter while in reality indulging in a distaste for Mendelism which had much deeper roots. So deep indeed did this distaste run that when, in 1916, Pearson was called upon to referee for the Royal Society the paper by Fisher which is now regarded as having finally reconciled continuous variation and Mendelian genetics, he wrote a most unenthusiastic report, suggesting finally that⁵¹

'Whether the paper be published or not should depend on Mendelian opinion as to the probability that Mendelians will accept in the near future a multiplicity of independent units not exhibiting dominance or coupling.

The result of this, as we shall see, is that Fisher's paper was not published until 1918, and then in the Proceedings of the Royal Society of Edinburgh. When Fisher sent Pearson a copy of the work in its published form, he still responded that 'I am afraid that I am not a believer in cumulative Mendelian factors as being the solution of the hereditary puzzle'.⁵² Fisher's work is now generally regarded as a work of genius, and as a magnificent contribution to genetics. Clearly, the biometric animus against the new Mendelian genetics went very deep indeed.

Finally, before going on to consider the interpretations of this debate which have been offered by various historians, it seems proper to say a little more about Bateson. He went on to a well-deserved reputation as establisher of the English Mendelian tradition, as the men who defended the embryonic discipline of Mendelian genetics from the onslaughts of the biometricians who, clearly, were intent on discrediting the Mendelian enterprise. After years of financial insecurity at Cambridge, he took up the position of director of the newly formed John Innes Institute for Horticultural Research in 1910. R.C. Punnett, his assistant, was then able to take up the Arthur Balfour chair of genetics when it was created by anonymous donation,⁵³ in Cambridge.

in 1912. As I have already mentioned, Bateson, was a hostile critic of the chromosome theory (i.e., the theory that Mendelian factors are located on the chromosomes) until 1921, writing, for example, in a review of The mechanism of Mendelian heredity by the great Americans Morgan, Sturtevant and Bridges, that⁵⁴

it is inconceivable that particles of chromatin or of any other substance, however complex, can possess those powers which must be assigned to our factors'.

Even after 1921, he seems to have been little attracted to the chromosome theory upon which so much American work had been based, and which had the power to explain the phenomena known to Bateson as coupling and repulsion, telling the cytologist Darlington that, as far as chromosome theory was concerned, he 'hated it' but supposed that 'we have to accept it all now'.⁵⁵

(B) Interpretations

These, briefly, are some of the main 'facts' of the history of the famous biometric-Mendelian debate, of the history of an episode when the biometric school of Pearson and Weldon came into conflict with the English Mendelians, and, in the process, lost in their attempt to become generally recognised as the leaders of a new wave of biologists equipped with the proper approach to the problems of evolution and heredity. These facts raise a genuine problem of historical interpretation, which may be expressed in several ways. We might say that the problem is that of explaining why, given a commonly available pool of data and information, Bateson and his colleagues should have become ardent Mendelians, while Pearson and Weldon did not, becoming in fact, in their differing degrees, hostile critics.

Another way of formulating the problem might begin by suggesting that, in science as in life, different people have different goals - they differ, along several dimensions perhaps, in respect of the type of theory which they desire to see established, and, unsurprisingly, are unreceptive, at least at first, to theories which are incompatible with their desires. Given the complex relations which, in logic, hold between theory and evidence in science - i.e., given, as Lakatos has put it, that theory and evidence cannot easily be forced into a two cornered fight - the upholders of different approaches may quite rationally sustain their approaches for a long time in the face of

a rival research programme. Hence, in this view of things, the historical problem becomes that of excavating the desires of the two groups, and, if possible, explaining why the two groups should have differed thus.

When addressing the following interpretations of the biometric Mendelian debate, it should be remembered that the power of Mendelism to explain a wide variety of genetic phenomena was not born with Mendelian theory. Mendel himself, could explain very little, and even T.H. Morgan was a non-Mendelian before 1910.⁵⁶ Similarly, in England, scepticism was the order of the day. E.B. Poulton, for example, writing in 1908, noted that many observed results threw 'doubt upon the extent of the application of Mendel's principles', and, in 1907, J. Arthur Thompson noted the existence of 'many exceptional results in Mendelian inheritance which suggest that the purity of the gametes is not so thorough-going as the theoretical Mendelian interpretation suggests.'⁵⁷

During the period we cover here, Mendelian genetics was a research programme which could explain only a small part of all hereditary phenomena. It was surrounded by countless apparent refutations. Naturally therefore, the explanations which we shall review concern mainly the important but neglected issue of why, in its embryonic phases, a research programme may look good and potentially fruitful to one group, but unattractive and wrongheaded to another.

Currently, there are perhaps three main explanations on offer, which I will very briefly outline, apologising in advance for any oversimplifications.

(1) Psychological

The first is due to William Provine who offers what might be called⁵⁸ a psychological approach, stressing the importance of interpersonal relations. The biometric-Mendelian debate is said to exemplify an historical pattern contradicting 'the current popular conception of science' - presumably the brands of folk-empiricism found in the introductions to text books.

Provine argues that, although, in logic, 'Pearson and Weldon might have argued that Mendelism supported Darwinian evolution', they did not do so because Bateson had made the 'obvious connection between

Mendelism and discontinuous evolution'. And, 'in reaction, the biometricians viewed Mendelism as a threat'.

But, why should this connection make Pearson and Weldon fight shy of Mendelism? A part of the answer is given when Provine cites correspondence⁵⁹ in which Bateson praises Pearson as able and as hard-working, and implores him not to become alienated from 'the work that is coming' on personal grounds. With Weldon though, things were different:

..as between him and me it is too late...At different times, as perhaps you know, we have each tried to renew our intercourse if not friendship but it came to nothing and it is no use trying again.

To which Pearson replied that,

I do not readily make friends, and when I say a man is a friend I mean I have tested the strength of his affection in the graver matters of life, and am prepared to do for him and accept from him anything that one human being can or will do for another.

And, says Provine, this clash illuminates the debate, for, 'it is evident that personality clashes were as important as scientific arguments in sustaining the conflict. If Weldon had adopted Mendelian inheritance, instead of opposing it, Pearson's attitude to Mendelism might have been different.' 60

Provine's model, it appears, is one of initial disagreement over continuity in evolution producing escalating bad feelings, transferred to Pearson, upholder of continuity and friend of Weldon. This, perhaps, pushed the continuity/discontinuity issue into the centre of an academic feud, with the biometric attitude to Mendelism becoming warped by its having first appeared within the enemy camp. Bad temper is seen as distorting the rational path.

(ii) Sociological

A second approach is due to Barnes and MacKenzie, who deploy a⁶¹ 'sociology of knowledge' approach, hoping thereby to stir up the historians. In practice, their approach extends only to Pearson and Bateson. They too see the continuity/discontinuity issue as crucial, as the core issue about which the debate hinged. Many of the individual disagreements in the debate, they argue, 'ultimately reduced to clashes between continuous and discontinuous views of evolution'.

In particular, it is said of Weldon and Pearson that 'since they saw that Mendelism required a discontinuist, mutationist theory to supplement it, they regarded it as unpromising to say the least'. Bateson as we know, was favourably disposed to Mendelism because he saw it as complementing his discontinuist views. For Barnes and MacKenzie, the explanatory problem reduces to that of explaining why, given access to common data and arguments, one side should have strongly favoured a continuous and the other a discontinuous view of evolution.

They discuss Provine's account, but reject it on the grounds that he fails to explain the initial parting of the ways with Bateson, and wish to go on to explain why the two sides took up their characteristic stances by relating these to 'theories or hypotheses about the incidence of beliefs'. And, in a series of arguments, which seek to epitomise the position of modern philosophers of science, which they attack, they conclude that the only satisfactory explanatory pathway for the historian is to explain belief in terms of something other than belief; they ask the historian to explain the intellectual stances of the scientists by reference to social rather than to intellectual or psychological factors. (My own view is that philosophers of science, concerned only with theory testing, even when acknowledging the Duhem-Quine thesis, have neglected to think about what makes a certain research programme attractive to a given man or group thereof.)⁶² They ask,⁶³

What primary social factors may determine different response to the same arguments, or the selection of different pathways from the vast number of intellectual moves which a scientist is always in a position to make?

What is to count as a social factor is not made clear, though, in practice, their paper seeks to link scientists' socio-political outlooks with their attitudes to continuity/discontinuity.

Expanding a theme due to Coleman,⁶⁴ Barnes and MacKenzie portray Bateson as an exponent of 'conservative thought', manifested for them in his elitist views and origins, his regard for a crypto-feudal social order, for compulsory Greek,^{and} for 'idealist' physics as practiced by Maxwell, Larmor, Lodge, Crookes and others. This tradition, Coleman has argued, may be seen as inclining Bateson to reject genetic materialism (the chromosome theory) in favour of genetic idealism (i.e., the vibratory theory of inheritance, analogous to the vortex theory of the atom.) Certainly, Bateson was a rampant, if incoherent advocate of

anti-egalitarianism, suspicious of any notion of human progress. Always fearful of the 'lawyer-politician type', Bateson even reacted against the eugenics movement, noting, with his fine, patrician, sneering style that:⁶⁵

Broadcloth, bank balances and the other appurtenances of the bay-tree type of righteousness are not really essentials of the eugenic ideal.

Having reviewed a range of Bateson's writings (all from his later life), Barnes and MacKenzie suggest that⁶⁶

his early discontinuous view of evolution accorded well with his social and political predilections, quite apart from its offering an alternative to a Darwinism which 'hued all too closely to the blighted atomistic individualism of the utilitarians'.

Pearson, by contrast, is portrayed as representing a very different tradition. Barnes and MacKenzie point out that this son of an upwardly socially mobile barrister had interests in evolution which, from the start, were those of a late-Victorian social theorist. They argue that his social Darwinism, his naturalistic ethics and his much broadcast advocacy of the gradual evolution of British society towards an elitist and collectivist state, based upon science in general, and upon eugenics in particular, make him as a man of a different stamp to Bateson: as someone much nearer to the Fabians, as a non-conservative thinker whose thought was permeated by 'ideas of continuity and gradualism'. This view may be supported by looking to his many essays on socialism, economics, Darwinism and eugenics.

Not denying that other factors operate (of which, more below) in the case of Pearson, Barnes and MacKenzie see this stress on gradual, non-revolutionary progress and on social continuity in Pearson's thought as one disposing him towards a continuist interpretation of evolution, and they argue as follows:⁶⁷

Since these very presuppositions played an important role in the social and political thinking of the protagonists, and there is a clear continuity between what we would regard as their scientific and their social thought, the conclusion is inescapable that their differences were sustained by concerns 'external' to science as we would define it, and must ultimately be grounded by reference to sociological determinants. Polarization around the continuous/discontinuous dichotomy was one kind of mediation between these determinants and natural science.

Their overall conclusion is thus:⁶⁸

Summarizing, we can claim to have shown that the Mendelian biometrician dispute cannot be understood in isolation from the cultural and political context of late-Victorian Britain. As the work of many historians has shown, there was an important link in the thought of this period between the belief that society was and should be in a state of gradual progress. We have characterised this link as a route via which general social factors influenced scientific debate. Similarly, we have shown how a view of evolution stressing the importance of (inpredictable) discontinuities was congenial to a conservative vision of society and polemically convenient to those who shared it.

(iii) Philosophical

In a published paper I have discussed Pearson's reactions to Mendelism, and have sought to explain it by reference to his philosophy of science and the conclusions he drew from it.

Pearson, as we have seen, was the author of a philosophy of science which combined some modern doctrines - e.g., the basis of knowledge in sensation, the unknowability of the thing in itself, the conception of scientific laws as 'economical summarisers' of observed routines in the passage of sensations, the necessity to eliminate metaphysics from science and its inability to count as knowledge - with strong Kantian overtones. He was a man who would suggest that,⁶⁹

the perceptive faculty may in itself determine largely
or in part the routine of our perceptions

He was, in short, a neo-Kantian instrumentalist.

He was, of course a neo-Kantian instrumentalist who contributed massively to the development of modern statistics, putting great emphasis on the theory of correlation, which, as noted, was a major feature of biometry. But, in the theory of correlation, he saw himself as advancing his Kantianism as well as his instrumentalism. For, he claimed that Kant had it wrong. Experience did not conform to the category of cause and effect, but to his new category of correlation. The world, he said, was inherently indeterministic,⁷⁰ and could be properly described only via the use of correlation method.

No phenomena are causal; all phenomena are contingent, and the problem before us is to measure the degree of this contingency, which we have seen is between the zero of independence and the unity of causation. That, briefly, is the wider outlook we must now take of the universe as we experience it.

From this premiss, he derived (invalidly) the conclusion that no theory would adequately cope with reality unless the classes of entities it posited were heterogeneous ones, showing variation about a mean. Thus, for example, we find him writing to Galton⁷¹ that,

On May 21st. I lecture to the Philosophical Club ... on, 'The possibility of a wider category than causation'. This lecture starts from the idea that no two physical entities are exactly alike, e.g., not even two atoms are precisely identical. They form a class with variation about a mean character. Hence even in physics the ultimate basis of knowledge is statistical - the category is of course correlation not causation.

And, later on, we find his extending this principle to Mendelism.⁷²

What if sameness and persistence be merely a relative distinction? What if the attempt of some biologists to replace vital variation by 'unit' characters be really a retrogressive change, and the persistency and absence of individuality to which they appeal as comparable with chemical changes be ultimately a false analogy, because the sameness of chemical theory is a statistical theory.

These views were first expressed in print only after the turning of the century and the advent of Mendelism. But, what makes them important is that by then they already had a place in Pearson's biological thought — in his theory of homotyposis, to be explained in the next chapter.⁷³

My explanation therefore, of his rejection of Mendelism, has been two-fold. I have attributed it to (i) his general distaste for theory (Mendelism posits real underlying 'factors'), especially in cases where, as in early Mendelism, it cannot cleanly explain (or 'describe') all the phenomena, arising from his instrumentalist and anti-metaphysical stance, and to (ii) his 'Kantian' views on homogeneous classes, which ruled out Mendelian theory in particular. For, the classes posited in that system did not exhibit variation about a mean point. All of their members were identical, except when, by some quantum-jump-like event, a mutation occurred. To Pearson who claimed that, in this respect, physics must learn from biology and that even, say sulphur atoms should be seen as a class showing variation, statistically arranged about some mean class-value, Mendelism was unattractive. It was inconsistent with the fruits of philosophy.

Barnes and MacKenzie have also noted Pearson's instrumentalism though not his "Kantianism". This they tend to attribute to his radical status, to his progressive commitment to a society based on hard, firm, objective scientific knowledge. We are dealing with the proponent of the Ethic of freethought, who saw science as the root to sound citizenship in the modern, progressive, 'socialist', eugenic state.

(c) Arbitration and synthesis.

These are some positions, which we may examine shortly. To start, however, there are some points of information which go a long way towards torpedoing, for good and all, the notion that there ever was anything that might with profit be called the biometric-Mendelian controversy, and which help clarify the nature of such dispute as really did exist.

(1)

I would like to produce this clarification via a listing of relevant points, which are as follows:

(1) Though the 'biometric school' was a large one - in the sense that many people trained in Pearson's laboratories, it would seem that many of the 'genuine' biologists amongst these trainees did not follow their leaders in the matter of hostility to Mendelism. We have already seen the example of A.D. Darbishire, who recanted after serving briefly as Weldon's assistant, and who went on to work as a lecturer in genetics and to write his book on Breeding and the Mendeli discovery. To the case of Darbishire must be added those of C.D. Davenport, whose career combined eugenics, biometrics and Mendelism⁷⁴ - for he became doyen of the American eugenics movement - and of Raymond Pearl the distinguished American biometrician who did not follow Pearson's strong anti-Mendelian path⁷⁵ As a consequence, Pearl was sacked from his editorial position on Biometrika. Indeed, among the 'genuine' biologists on Pearson's staff, only David Heron seems to have maintained a long-lived opposition to Mendelism. At this stage it is also appropriate to mention G.U.Yule, who, on several⁷⁶ occasions, argued publicly that observed biometric results could indeed be explained within the Mendelian explanatory schematism.

(2) The 'Mendelians' showed analogous in-group differences, but

of a different order. Some of these have already been noted, particularly the matter of the chromosome theory which Bateson rejected, but which other men - like Doncaster and Lock - accepted⁷⁷ whole heartedly. More relevant in the present context is the matter of views on discontinuity in genetics and evolution. Here, it seems to me, there has been a great deal of confusion engendered, of which some is due to the Mendelians themselves, and some to historians who have attempted to document and explain their views. To clarify this matter, I would like to distinguish between two separate doctrines of discontinuity in the Mendelian camp, which I shall refer to as 'Discontinuity₁' and 'Discontinuity₂'.

'Discontinuity₁' is simply the doctrine that all forms of variation, barring the sorts of genuinely environmental 'fluctuations' uncovered in the work of Johannsen and others,⁷⁸ would be shown to be controlled by a number of Mendelian factors, which whether of large or small phenotypic effect, were discontinuous in the sense that they did not blend in heredity. One expression of this was Bateson's early scheme for explaining Gaussian variation along Mendelian lines. Another was a passage in the works of 'Ardent Mendelian', who held that:⁷⁹

Our present knowledge renders it easy for us to conceive of the existence of segregation without their being any obvious manifestation of its existence.

This being the case, it is hardly surprising that Mendelians like Lock responded favourably to Yule's claim that Gaussian variation and Mendelism were quite compatible.⁸⁰ It was a consequence of the doctrine of 'Discontinuity₁' that this should be the case.

'Discontinuity₂' is a separate doctrine - which was never very clearly spelled out and never very well developed. It is the doctrine that, in general, evolutionary change was brought about by large discontinuous variations, subsequently inherited in the Mendelian fashion. This was a doctrine more strongly supported by Bateson and his colleague Punnett than by other British Mendelians, and, as I have noted, arose from a belief on the part of Bateson and Punnett that it was implausible to regard the contemporary distribution of species with their distinguishing characteristics as the

outcome of series of gradual changes from forms having marginally less to forms having marginally more 'adaptation' or 'fitness' on account of these changes. In these circumstances, it was simpler to suppose that change was due to sudden large discontinuities, often of no great adaptive benefit. Punnett expressed nicely the feeling of relief that such a doctrine could offer when he wrote that it released the biologist⁸¹

from the burden of discovering a utilitarian motive behind all the multitudinous characters of living organisms.

'Discontinuity₂' therefore, was a doctrine based on objections to a view of nature in which all change and diversity had its utilitarian side and *raison d'être*. It was the biological equivalent of a denial that all features of, say, human culture, could be explained in terms of advantage conferred upon their exponents. It was, as I have said, not a doctrine which seems to have been strongly held by all British Mendelians.

(3) Now comes a point about Pearson and Weldon - the high command of the biometric school. For, it seems that they were not so united in their thinking as might be supposed - though, of course, they were hostile to 'Discontinuity₂', a doctrine which their positivistic reduction of terms like 'adaptation' made it hard for them even to appreciate. The text of Weldon's book on heredity, (see appendix) unpublished on his death, reveals that his interpretation of the biometricians' major scientific formula - the law of ancestral heredity - was quite different to Pearson's. Pearson, we have seen, denied it any physiological interpretation, and, in his life of Galton, went on, quite unconvincingly, to attribute a similar view to Galton. But Weldon did favour a physiological interpretation, based on Galton's 'stirp' theory of inheritance, in its turn, a development of Darwin's 'pangenesis'.⁸² As De Marrais has noted, this⁸³ differed from the Mendelian view in the following crucial respect. For a Mendelian, only one or two alternative traits is made available to an offspring by a parent. For the Galtonian, however, 'the peculiarities of each ancestor may be preserved, in potentia, in each successive generation'. Weldon, we find, interpreted Pearson's ancestral law thus, seeing its coefficients as giving the proportions in which 'dominant elements which are distributed among the individuals

of a generation and determine their visible characters, are derived from the dominant determinants of parents, grandparents and remote ancestors'. Weldon, it appears was a Galtonian, and we can certainly interpret his characteristic objections to Mendelism in this light. In a paper incomplete on his demise, he sought to display Mendelian results of breeding experiments as due to important, but special cases of the Galtonian theoretical set-up, and not to the correctness of the Mendelians' ontology. Full documentation is given in Appendix

(ii)

Finally, I would like to offer a new perspective on the debate, utilising the information of the various parts of this paper, and giving particular attention to Barnes and MacKenzie's important attempt to interpret the debate in accordance with the canons of the sociology of knowledge.

The debate was started by Weldon and Bateson, and came to a head over the issue of continuity versus discontinuity in evolution. But it had a deeper root in the research programmes of the two workers, although the precise nature of these is hard to reconstruct. Bateson's main aim appears to have been to understand the mechanical causes of pattern and variation within the organism. Coleman's work and Bateson's own Problems of genetics make this fairly clear. The writing is obscure, but there is little reason to doubt that Bateson's early position, stemming from his attempts to explain metameric segmentation, differed little from his later one. Discontinuity was embedded in a rich theoretical matrix, leading, as we have seen to analogies with mackerel skies, chladni plates, and so on. When these analogies were appreciated, said Bateson, 'no one will be astonished at, or reluctant to admit, the reality of discontinuity in variation'.⁸⁴ Thus we can begin to understand his remark of late life that Mendelism had been something of a diversion. To the very end, Crowther relates, Bateson was frustrated by his inability to discover the causes of variation, which always appeared 'utterly mysterious'. Weldon's thought is also opaque, but in his case too, the issue of continuity versus discontinuity was tied in with broad-ranging theoretical beliefs - in the propriety of a mass, statistical study of evolution, and in a Galtonian theory of heredity, which, at the end of his life, was leading him to articulate

principles of dominance relevant to his Galtonian quest by discussion of recent work in regeneration and development. Clearly, a great deal was at stake in the debate between Bateson and Weldon, which was one between an idiosyncratic Mendelian and the follower of a non-Mendelian, rival, physiological theory of heredity, surrounded, in its proposer's mind, by a rich variety of thoughts on research goals and style. The fact that all of Bateson's group did not share all his views can be seen by looking to their works. Doncaster, for example, was an early follower of the chromosome theory.⁸⁵

Pearson, as I have noted, and as Pearson make clear himself in a letter to the Manchester Guardian ⁸⁶ entered biology as a social Darwinian. Unsurprisingly, he focused on continuously varying characters, and had little to say on major biological issues like, say, speciation. He appears generally hostile at all times to saltatory theories of evolution, but not in a striking way. He was an attested instrumentalist of neo-Kantian tendencies whose philosophy of science had been formed at an early stage - in 1879, whilst a Heidelberg student, I would suggest.⁸⁷ His instrumentalism was built into his biosocial research programme, frequently described as non-theoretical.

His instrumentalism, as noted above, was accompanied by a curiously derived doctrine of heterogeneous classes, demanding even that, say, sulphur atoms, vary about some mean. His opposition to Mendelism was milder than Weldon's, passive rather than active, and is best described as one of disapproving scepticism laced with contempt for the undoubtedly ad hoc moves made by some Mendelians. In Pearson, I suggest, we have a third man with a third research programme, shaped and guided by the various directives indicated, which, collectively, prejudiced him against theory in general and to theories of the Mendelian type in particular. His views on correlation show him engaged in a broader research programme than his biometric one in which he argued for universal indeterminacy and the desirability of mathematical statistics as a universal scientific methodology. Mendelism conflicted with these cosmic ambitions.

I suggest, therefore, that we can better understand his reaction to Mendelism in these terms than in terms of the supposedly socially mediated objection to evolutionary discontinuity proposed by Barnes and MacKenzie. There seems little doubt that he was hostile to saltatory evolution, but there is little reason to suppose that this would have prejudiced him against Mendelism, for he knew that Mendelism and continuity in variation were compatible. The discontinuities of the Mendelian multifactorial approach are small, small as one likes, and hardly the biological analogues of social revolution. If this were insufficient, there remains the doctrine of varying atoms mentioned above. It is hard to suppose that this may be understood in terms of a tie-up between a belief that society should be in a state of gradual progress and the belief that natural processes were uniform, slow and continuous - especially when we have an apparent strong counter-example in the shape of Charles Lyell the geologist, the uniformitarian par excellence, whose views have recently and convincingly been related to his conservative outlook by Michael Bartholomew.⁸⁸ Similarly, the attempt to ground Pearson's philosophy in social factors, via a suggestion that his concerns were simultaneously political and epistemological seems besides the point, partly for reasons discussed below, partly because there was no general connection in late Victorian society between liberal progressivism and a Pearsonian style of positivism.

Hence, in this view, the biometric-Mendelian debate reduces to the interactions of three powerful academics: two of them joined by strong personal and ambiguous intellectual bonds. The pupils of Pearson and Weldon did not see incompatibilities between Mendelism and biometric results because they were unencumbered by the background commitments of the leading men.

This, of course, just describes some main programmes and the background assumptions built into them. We are still faced with the issue of explaining why the combatants should have advanced with enthusiasm, the sorts of research programmes which they did advance. Here, perhaps, the explanatory strategy of Barnes and

MacKenzie may assist. This, at first sight, appears to be a strategy of showing that, in selected cases, the basic propositions of a scientist's research programme may be displayed as especially attractive to him on account of a certain harmony between these propositions and the attitudes, aims and goals associated with his social situation and relationships. (Thus, for example, we would be unsurprised to find a pacifist opposing accounts of nature emphasising its innately pugnacious aspects.) In fact, however, Barnes and MacKenzie's position is interestingly stronger. They argue that it is only by explaining intellectual positions in terms of 'social' factors that an infinite regress within historical explanation can be avoided.⁸⁹

At the end of it all, we still have to ask why the opposing sides sustained conflicting sets of beliefs. We may know more about the controversy but we are as far from explaining it as ever. However much we study what is believed we cannot expect to lay bare why it is believed. For this we must go beyond the beliefs themselves, and relate the controversy to theories or hypotheses about the incidence of beliefs.

They go on to insist on social explanation as the only way to cut the regress, dismissing the other main possible strategy for so doing which they perceive - namely that of distinguishing between rational and irrational beliefs and explaining them differently - on philosophical grounds. Whatever the merits of these grounds, their detection of a possible infinite regress within the process of explaining beliefs or attitudes towards a new theory by the citation of other beliefs is clearly meritorious..

The question, however, is that of whether genuinely social explanations can be adduced in such a form as to cut the regress. It seems doubtful that they can, at least for cases relevantly similar to the present one. This can be seen by focussing in upon Barnes and MacKenzie's treatments of Bateson and Pearson, putting aside for the moment the doubts previously expressed concerning the possibility of explaining Pearson's rejection of discontinuity and his rejection of Mendelism with one and the same explanatory factor.

Here we have two men who developed socio-political outlooks quite out of harmony with the traditions of their backgrounds. Bateson, it appears, did fall into a conservative and aristocratic outlook which led him to sneer at the utilitarian metaphysics of

But this was not the natural path for a scion of St. John's College Cambridge, described by Crowther as the headquarters of the 'spirit of the liberal rentiers' - who was to fall out with Sedgwick of Trinity (headquarters of the 'Conservative landed aristocracy', and who was the son of a noted liberal.⁹¹

Pearson, similarly - the son of a lawyer and a fellow of King's College Cambridge - was hardly following following any well laid down tradition or persons of his background when he espoused the causes which he did espouse.

The point, surely, is that, though we may do sterling work in explaining why one scientist finds a research programme attractive and plausible by showing harmonies between the research programme's core intellectual content and his broader social, political, philosophical or other orientations, we have hardly provided a social explanation and thereby cut the feared explanatory regress if these views are not the natural views for persons of the types involved, but are, rather, sets of views which they have gone out of their way to adopt. In the cases of Pearson and of Bateson, we are dealing with men who deliberately recruited themselves into new views, not in the least bit natural for men of their social position. Since it is these views which are presented as explainers of Bateson's and Pearson's scientific positions, their citation does not perform the desired role of social explanation - i.e., that of severing an explanatory regress. For, we have still to provide a social explanation of why Pearson and Bateson should have taken up these socio-political views. Barnes and MacKenzie seem aware of this problem, but have, apparently, no answer beyond a general view that the sorts and styles of socio-political thinking which individuals are prone to recruit themselves into are loosely constrained by socio-economic factors. Thus,⁹²

There need be no necessity in the chain of affiliations which make up an individual biography. But the links in the chain, the affiliations themselves, must be selected from given possibilities. Primary social and economic causes, in changing the nature and distribution of institutionalised belief, change the range of these given possibilities.

This is interesting, but not very helpful. For, supposing that we produced a good, convincing theory about the ways in which

'primary social and economic causes' change the range of possible affiliations for the actor, then, unless the number of affiliations allowed at any time can be shown to be small, and each possible affiliation be shown to be a tightly drawn one, there would seem to be little chance for anything approaching the strong hard form of social explanation which Barnes and MacKenzie look for. In proportion as the number of possible affiliations be large, or, insofar as the affiliations be loose ones, then the plausibility of claiming that the sorts of perspectives used by Barnes and MacKenzie are genuine social explanations seem to diminish. Take Pearson as an example: if we suppose that the late 19th century contained a 'progressive' group, generally believing in slow and gradual social change, then, unless we know that the group was extremely zealous about discontinuity, we shall find the explanation of Pearson's rejection of Mendelism on the basis of such membership unsatisfying. For, we have seen that he knew that Mendelism and apparent continuity were quite compatible. Similarly, unless the joining of such a 'progressivist' group was fairly normal under the circumstances, then the explaining of Pearson's choice of affiliation seems to raise all the problems which social explanation was invoked to alleviate. Neither of these things seem to be the case.

It is not that the general model which Barnes and MacKenzie seem to promote (i.e. that of possible intellectual grouping being loosely constrained by economic and social conditions, and of membership in one of these groups inclining the scientist to wish to promote one type of theory rather than some other) is at all implausible. It is rather that, at present, we have no good theory of restraints, and that, if the present example be typical, membership of such groups seems insufficient to explain reactions to quite complicated scientific ideas. Furthermore, when dealing with a small number of scientists, all of the Cambridge fellows of approximately the same period, the task of explaining why each of them opted to join a different intellectual group can be extremely difficult - and, certainly, not easily done via social explanation. Possibly a psychoanalytical approach would be better under such circumstances, but, again, the problem is that there appears to be no suitable theory extant.

APPENDIX J . THE RATIONALE FOR WELDON'S OPPOSITION TO MENDELISM

In the body of the *Chapter*, I have suggested that Weldon's opposition to Mendelism was based upon idiosyncratic considerations - notably a commitment to a Galtonian theory of inheritance. In this appendix, I will briefly mention the main thrusts of his opposition to Mendelism and will show how they relate to his theoretical position.

First, let us consider the objections to Mendelism. The crucial one was embedded in Weldon's claim that,⁹³

The fundamental mistake which vitiates all work based on Mendel's method is the neglect of ancestry, and the attempt to regard the whole effect upon offspring, produced by a particular parent, as due to the existence in the parent of particular structural characters; while the contradictory results obtained by those who have observed the offspring of parents apparently identical in certain characters show clearly enough that not only the parents themselves, but their race, that is their ancestry, must be taken into account before the result of pairing them can be predicted.

Typically, Weldon claimed that Mendel's work on peas gave,⁹⁴

an admirable illustration of the effect produced by crossing a few pairs of plants of known ancestry; but while they show this perhaps better than any similar experiment, they do not afford the data necessary for a statement as to the behaviour of yellow-seeded peas in general, whatever their ancestry, when crossed with green-seeded peas of any ancestry.

And, this went hand in hand with an objection to Mendelian doctrine that the extracted recessive was gametically pure, and with a claim that, in striking instances, it was improper to regard, say, 'round' and 'wrinkled' peas as forming two classes each of which was quite distinct from the other and was genetically homogeneous. Examples were provided from breeders' records and from breeders' seed stocks.

In the extract from Weldon's unpublished book reproduced below, we begin to see the sorts of theoretical considerations which underlay and motivated this sort of objection to Mendelism. The extract speaks eloquently for itself, though I have added emphasis to what I consider as important points, together with commentary in the form of notes. Possibly the central point is De Marrais' point that, for Galtonians,⁹⁵

the peculiarities of each ancestor may be preserved, in potentia, in each successive generation.

whereas, for Mendelians, only one of two alternative traits may be made available to the offspring by a parent. We see this reflected in Weldon's discussion of the Galtonian scheme, which he obviously endorses, when he writes that,⁹⁶

a particular element achieves dominance partly through being more vigorous than its immediate competitors, and partly through the unknown conditions under which the struggle takes place.

The thrust of his research was to uncover the laws which governed the outcome of this struggle - see for example, the interpretation given the 'law of ancestral heredity' in the text.

The only detailed working out of Weldon's ideas was presented in a posthumous paper, constructed by Pearson on the basis of notes left by Weldon.⁹⁷ We can see in this work something of the basis upon which Weldon's hostility to the idea of the purity of extracted recessives was based. The full refinements of the theory, due, one imagines, largely to Pearson may perhaps be disregarded for the moment, in favour of looking at one of the simpler cases envisaged by Weldon.

In Weldon's scheme for heredity, it was supposed that a given character would be controlled by a number of determinants, located on a pair of homologous chromosomes. Let us consider the case when there are just two determinants. And, with Weldon, let us consider the result of crossing members of two 'pure' races - an 'allogenic' race with four 'allogenic' determinants (two on each chromosome), say AAAA; and a 'protogenic' race, with four 'protogenic' determinants, say aaaa.

Crossing the two would yield a hybrid race, each of whose members had a pair of chromosomes - one with the determinantal constitution AA, and the other with aa.

Now comes the crucial move, the great difference from Mendel's scheme. Weldon envisaged the reduction division as a process of the fusion of chromosomal contents - giving AAaa - followed by the formation of haploid germ cells, of different types, whose frequencies were obtained by a process of random pair-selection of determinants from the determinantal set AAaa. Simple combinatorial arithmetic shows that,

in this case, the germ cells of different constitutions (gametes) would be formed in the following ratios.

Gametes with two allogenic deter- minants. (AA)	Gametes with one allogenic, one protogenic deter- minant (Aa)	Gametes with two protogenic deter- minants (aa)
---	--	---

1 : 4 : 1

And, on crossing, we get the following array produced

zygotes with 4 allogenic determinants	zygotes with 3 allogenic, 1 protogenic determinant.	zygotes with 2 allogenic, 2 protogenic determinants	zygotes with 1 allogenic, 3 protogenic determinants	zygotes with 4 protogenic determinants
---	--	--	--	---

1 : 8 : 18 : 8 : 1

If we group the first two classes, and the last two, then we have a 1:2:1 ratio. And, as Pearson wrote in his exposition of Weldon's ideas:

This case is of peculiar interest; we get absolutely the Mendelian percentages, if the somatic character follows the preponderance of a given pure race determinant, using preponderance here in a simple numerical sense. Further, the protogenic 25 per cent, would apparently breed true to the somatic character of the pure protogenic race, if the somatic character of the balanced heterozygote were, as is occasionally asserted to be the case, indistinguishable from that of the protogenic race. The peculiar suggestiveness of this result lies in the exact Mendelian properties arising on a simple view of dominance apart from any hypothesis of the pure gamete. There exists a latent allegenic determinant in the heterogenic chromosome of a large percentage of the 25 per cent, with dominant protogenic character. This, if judicious cross-breeding were adopted, might be rendered manifest in some, if only a small number, of the grandchildren of the offspring of the hybrids.

I take it that what Pearson was suggesting, on behalf of Weldon, that here was an instance in which a set of a set of individuals, seen by the Mendelians as genetically homogeneous, were in fact genetically heterogeneous - though, as he noted, this 'if judicious cross-breeding were adopted, might be rendered manifest in some, if only a small number, of the grandchildren of the offspring of the hybrids'. This, I take it, is a reflection of Weldon's general suspiciousness of the

Mendelian view that extracted recessives were genetically identical to the members of a pure-breeding recessive strain.

Further developments of Weldon's theory were obtained by increasing the number of determinants held to control any given character, giving an interesting range of ratios. The main details are fairly easy to follow, and were laid out by in Pearson's exposition. But, the point which remains after even so brief an exposition (certainly all that is needed for present purposes) is that Weldon's opposition to Mendelism must be understood in terms of inheritance. There is little reason to suppose that Pearson himself, or anyone else, put much faith in this theoretical approach. He was, on his own admission, assisting Weldon with the theory at the time of Weldon's death, but, as he remarked, his contribution was 'solely the mathematical analysis'.

.....
2, Tilney Street,
Mayfair W.

March 7th. 1912

Dear Vice - chancellor,

Towards the end of last year there was a meeting at Mr. Balfour's house in Carlton Gardens of a few representatives members of the University of Cambridge interested in the subject of genetics. The meeting had under consideration a short paper written by Mr. Balfour in July 1910 which dealt with the endowment of the study of genetics in the University of Cambridge. As a result of that meeting I am glad to be able to inform you that a generous benefactor, who stipulates that his name shall not be mentioned, has placed in my hands a sum of twenty thousand pounds (£20,000) for the purpose of endowing a professorship at Cambridge in connection with the experimental study of heredity and development by descent.

It is stipulated that the new chair shall be called the Balfour chair of genetics.

There are two more subsidiary (sic) conditions which I am to place before you for your consideration.

(1) That the first appointment should be made jointly by the Prime-Minister and Mr. Balfour.

(2) That the regulations governing future appointments to and functions of the chair shall be submitted through me to the anonymous benefactor before the endowment fund is placed in your hands.

I am able further to state that our generous benefactor is willing to furnish such funds as may be necessary to provide and equip a small station at Cambridge for the use of the professor should such a course be considered desirable after careful examination of the methods likely to be most satisfactory for the purpose of research in the domain of geneti

I remain,
yours sincerely,
Esher.

- 1 Cited in E.S.Pearson, Karl Pearson: An appreciation of some aspects of his life and work, Cambridge (1938), 34.
- 2 See A.G.Cock, 'William Bateson, Mendelism and biometry', Jnl.hist.biol. 6 (1973), 1 - 36. L.Farrall, 'The role of controversy and conflict in science: A case-study - the English Biometric School and Mendel's laws', Social studies of science, 5 (1975), 269 - 301. P.Froggatt and N.C.Nevin, 'The law of ancestral heredity and the Mendelian-biometrician controversy in England, 1889 - 1906', Jnl. med. genetics, 8 (1971), 1 - 36. Also, by the same authors, 'Galton's law of ancestral heredity and its influence on the early development of genetics', History of science, 10 (1971), 1 - 27. W.Provine, The origins of theoretical population genetics, Chicago, (1971).
- 3 For details, see B.Bateson, William Bateson F.R.S., Cambridge (1928). See also, J.G.Crowther, 'William Bateson, 1861 - 1926', in his British scientists of the twentieth century, London (1952).
4. For an account of the life and work of Balfour, see G.L.Geison, 'Sir Michael Foster and the rise of the Cambridge School of physiology, 1870 - 1900, Unpublished Yale Ph.D. thesis (1970). Balfour is perhaps most famous for his Treatise on comparative embryology, 2 vols., London (1880)
- 5 For details of the overall quest, see Charles Singer, A history of biology, 3rd. edn, London (1959). See chapter 13, 'Development of the individual'.
- 6 William Bateson, 'On the ancestry of the Chordata', Quarterly jnl.microscopical science, 26 (1886), 535 - 572.
- 7 See B.Bateson, op.cit. (note 3), 102.
- 8 W.Bateson, Materials for the study of variation, London (1884).
- 9 See B.Bateson, op.cit. (note 3) 42.
- 10 Bateson, op.cit. (note 8), 10
- 11 Ibid, 10 - 13.
- 12 Ibid, 572 - 573.
- 13 T.H.Huxley, 'On natural history, as knowledge, discipline and power', Reprinted in vol.1. of ed. E.Ray Lankester, Scientific memoirs of T.H.Huxley, London (1898). See p. 311.
- 14 This has been most fully expounded in William Coleman's paper, 'Bateson and chromosomes: Conservative thought in science', Centaurus, 15 (1970), 228 - 314.
- 15 William Bateson, The problems of genetics, New Haven (1913), 35 - 36.
- 16 See chapter 3.
- 17 See Bateson, op.cit. (note 8). Modern discussions of such issues are

to be found in T.H.Frazetta, Complex adaptations in evolving populations. Massachusetts (1975). See especially chapter 4 on 'Evolving the improbable'. See also, N.MacBeth, Darwin retried, London (1974). See especially chapter 17, 'The case of the hopeful monster'.

- 18 W.F.R.Weldon, 'Remarks on variation in animals and plants', Proc.Roy Soc., 57, (1895), 379 - 382.
- 19 E.S.Pearson, op.cit. (note 1), 27.
- 20 W.F.R.Weldon, 'The study of animal variation', Nature, 50 (1894), 25-26.
- 21 An account of this debate is given in Provine, op.cit. (note 2), 45-48. Sir William Thiselton-Dyer (1843 - 1928) was a leading academic botanist who was to serve as director of Kew Gardens from 1885 to 1905
- 22 Cited in the 'Selection from the letters and ephemeral writings of William Bateson', unpublished typescript by A.G.Cock, Department of biology, University of Southampton. See his section 4c on the Cineraria controversy.
- 23 Described in Provine, op.cit. (footnote 2), 48 -56. The paper in question was W.F.R.Weldon's 'Attempt to measure the death rate due to the selective destruction of Carcinus moenas with respect to a particular dimension', Proc. Roy.Soc., 58 (1895), 360 - 379.
- 24 Provine, op.cit (note 1), 48 - 56.
- 25 W.Bateson, 'Heredity, differentiation and other conceptions of biology: A consideration of Professor Karl Pearson's paper 'On the principle of homotypy' ' Proc.Roy.Soc., 69 (1901). See also , Provine op.cit. (note 1), 61.
- 26 K.Pearson, 'On the fundamental conceptions of biology', Biometrika, 1, (1902), 320 - 344.
- 27 W.Bateson, 'Variation and differentiation in parts and brethren', privately printed and circulated, July 1903.
- 28 For details, see R.Olby, Origins of Mendelism, London (1966).
- 29 For details of the meeting with Punnett, see Cock, op.cit. (note 22). See section 5, 'The Bateson-Punnett partnership', which shows that Bateson was more favourably disposed to Leonard Doncaster than to Punnett, but that Doncaster turned down Bateson's offer of an assistantship in early 1904. Further details of the early days of British Mendelism are given in R.C.Punnett, 'Early days of genetics', Heredity, 4 (1950), 1 - 10
- 30 See Reports to the Evolution Committee of the Royal Society
- 31 See, e.g., W.F.R.Weldon's 'Mr Bateson's revisions of Mendel's theory of heredity', Biometrika , 2 (1903), 286 - 298. A full list of Weldon's publications is given in K.Pearson, 'W.F.R.Weldon, 1860 -1906', Biometrika, 5 (1906), 1 - 52.
- 32 See Punnett, op.cit. (footnote 29).
- 33 See Provine, op.cit (note 2), 79. Provine refers to Pearson's attack on Darbishire in Nature, 70 (1904), 530.

- 34 See Punnett, op.cit. (note 29)
- 35 See I Lakatos, 'Falsification and the methodology of scientific research programmes', in eds. I. Lakatos and A. Musgrave, Criticism and the growth of knowledge, Cambridge (1977).
- 36 For details of Bateson's work, see the Reports to the Evolution Committee of the Royal Society. But, see also, E.A. Carlson, The gene; A critical history, London (1966). See, in particular, Chapter 7, 'Crossing over versus reduplication'. For details of Bateson's 'presence and absence' theory of the gene, see R.G. Swinburne, 'The presence and absence theory', Annals of science, 18 (1962), 131 - 145.
- 37 For details of the emergence of the Morgan school, see A.H. Sturtevant, A history of genetics, New York (1965). Also, L.C. Dunn, A short history of genetics, New York (1965). For Bateson's review of the Mechanism of Mendelian heredity by T.H. Morgan, A.H. Sturtevant, H.J. Muller and C.B. Bridges, New York (1915), which gave a clear exposition of the chromosome theory of the gene, see W. Bateson, 'The mechanism of Mendelian heredity', Science, N.S. 44 (1916), 452 - 463.
- 38 For details, see Provine, op.cit (note 2), chapter 4.
- 39 See D. Heron, 'Mendelism and the problem of mental defect. 1. A criticism of recent American work', Questions of the day and fray No 7 London, Eugenics Laboratory, (1913). It is plain to see that Pearson's influence lay behind the hand of Heron.
- 40 See Pearl correspondence, Pearson Archive, University College London.
- 41 'Ardent Mendelian', 'Methods and results', Mendel journal No 1 (1909), 159 - 194. See pp. 159 - 161.
- 42 See K. Pearson and A. Lee, 'On the laws of inheritance in man. 1. Inheritance of physical characters', Biometrika, 2, (1903), 357 - 462.
- 43 K. Pearson, 'On a generalised theory of alternative inheritance with special reference to Mendel's laws', Phil. Trans Roy. Soc., 203A (1904), 53 - 86.
- 44 For an account of this episode, see B. Norton, 'Metaphysics and population genetics. Karl Pearson and the background to R.A. Fisher's multifactorial theory of inheritance', Annals of science, 32 (1975) 537 - 553.
- 45 See G.U. Yule, 'On the theory of inheritance of quantitative compound characters on the basis of Mendel's laws - A preliminary note', Report of the third international conference on genetics, 140 - 142, London (1907)
- 46 K. Pearson, 'The ancestral gametic correlations of a Mendelian population mating at random', Proc. Roy. Soc., 81B (1909), 225 - 229. See also Norton, op.cit. (footnote 44).
- 47 Ibid.
- 48 This point is clearly made in Cock, op.cit. (note 2).

- 49 W.Bateson, Mendel's principles of heredity ,Cambridge(1909), 76.
- 50 In the Pearson archive, University College London.
- 51 Reported in B.Norton and E.S.Pearson, 'A note on the background to and refereeing of Sir R.A.Fisher's 1918 paper "On the correlation between relatives on the supposition of Mendelian inheritance"', Notes and records of the Royal Society of London, 31 (1976), 151 - 162.
- 52 Cited in E.S.Pearson, 'Studies in the history of probability and statistics.20. Some early correspondence between W.S.Gossett, R.A.Fisher and Karl Pearson, with notes and comments', Biometrika, 55 (1968), 445 - 457.
- 53 Bateson went on to a very distinguished career, and was to be the major figure in the formation of the Genetical Society. See, e.g., D.Lewis, 'The Genetical Society - The first fifty years', in ed. J. Jinks, Fifty years of genetics, Edinburgh (1969). The circumstances of the creation of the Arthur Balfour chair remain obscure, but, in the Appendix to the chapter, I cite the text of a letter from Lord Esher to the Vice Chancellor of Cambridge University, dated March 1912, outlining the conditions under which an anonymous donor would set up the chair.
- 54 Bateson, op.cit. (Note 37).
- 55 Coleman, op.cit. (note 14),262.
- 56 For an account, see Garland Allen, 'T.H.Morgan and the problem of natural selection', Jnl.hist. biology,1 (1968),113 - 139.
- 57 See E.B.Poulton, Essays on evolution ,1889 - 1907, Oxford (1908),xxxv.
- 58 op.cit. (note 2), chapters 2 and 3.
- 59 Ibid, 63.
- 60 Ibid, 64.
- 61 D.MacKenzie and S.B.Barnes, 'Historical and sociological analyses of scientific change:The case of the Mendelian - biometrician controversy in England', typescript available from Science Studies Unit,Buccleuch Place, University of Edinburgh.
- 62 Popper, for example, though writing on the 'Logic of scientific discovery', has almost nothing to say on the topic of where of where hypotheses come from.
- 63 Barnes and MacKenzie, op.cit. (note 61).
- 64 Coleman, in op.cit.(note 37) , portrays Bateson as a man whose whole thought orientation conformed to the mould of 'conservative thought' as outlined in Karl Mannheim's essay on 'Conservative thought' in his Essays on sociology and social psychology , London (1953). Further discussion of the nature of 'conservative thought' is to be found in Jon.Harwood's 'The race-intelligence controversy:A sociological approach - 1 Professional factors', Social studies of science , 6, (1976), 369 - 394.

- 65 W.Bateson, in B.Bateson, op.cit (note 3),375. This is an extract from Bateson's Galton lecture for 1909, 'Commonsense in racial problems'
- 66 Barnes and MacKenzie, op.cit. (note 61)
- 67 Ibid.
- 68 Ibid.
- 69 See chapter 4.
- 70 Karl Pearson, The grammar of science , 3rd. edn. London (1911), 174.
- 71 Cited in Karl Pearson, The life, letters and labours of Francis Galton, Cambridge (1914 - 1930), vol 2a, 314.
- 72 Pearson, op.cit. (note 70)
- 73 See chapter 6.
- 74 For details, see M.Haller, Eugenics:Hereditarian attitudes in American thought, New Brunswick (1963), 63 - 75. Also, K. Ludmerer, Genetics and American Society, Baltimore (1972).
- 75 For details of R.Pearl, see C.C.Gillispie, Dictionary of scientific biography, 10 (1974), 235 - 242.
- 76 See, e.g., G.U.Yule, 'Mendel's laws and their probable relations to intra-racial heredity', New Phytologist , 1 (1902), 193 - 207, 222 - 238.
- 77 See, for example, R.H.Lock, Recent progress in the study of variation ,heredity and evolution , 5th. edn.,(London (1920). See chapter , 9,'Recent cytology'.See also L.Doncaster, Heredity in the light of recent research, Cambridge (1910). See Appendix 2, 'The material basis of inheritance'.
- 78 See W.Johannsen, Ueber Erblichkeit in Populationen und in reinen Linien, Jena (1903)
- 79 Op.cit. (note 41)
- 80 R.H.Lock, op.cit. (note 77), 227 et seq.
- 81 R.C.Punnett, Mendelism , 4th. edn., London (1912), 137.
- 82 See chapter 3.
- 83 R.De Marrais, 'The double-edged effect of Sir Francis Galton: A search for motives in the Biometrician-Mendelian debate', Jnl. hist.biol. ,7, (1974),141 - 174.
- 84 Bateson, op.cit. (note 15).See extract quoted in text.
- 85 See Doncaster, op.cit. (note 77),
- 86 See chapter 4 (note 58)
- 87 See chapter 4

- 88 M.J.Bartholomew, 'Lyell and evolution:An account of Lyell's response to the prospect of an evolutionary ancestry for aman', Brit.jnl. hist. science ,6, (1972), 261 - 303.
- 89 Barnes and MacKenzie, op.cit.(note 61)
- 90 This is made quite clear in Coleman, op.cit. (note 14)
- 91 J.G.Crowther, op.cit. (note 3), 256.
- 92 Barnes and MacKenzie, op.cit. (note 61).
- 93 W.F.R.Weldon, 'Mendel's laws of alternative inheritance in peas', Biometrika,1 (1902), 252
- 94 Ibid.
- 95 R.De Marrais, op.cit. (note 83)
- 96 From Weldon's notes, kept in the Pearson Archive, University College London.
- 97 K.Pearson, 'On a mathematical theory of determinantal inheritance, from suggestions and notes of the late W.F.R.Weldon', Biometrika, 6 (1908).
- 98 Kept in the Cambridge University Library, reference number C U R 39 - .

Chapter 6. The rise and fall of the law of ancestral heredity.

Introduction

In the work so far, the 'law of ancestral inheritance' has put in several appearances, though, in the main, in two contexts - (i) the work of Galton himself, and (ii), the work of the biometric school, whose members presented this 'law' as the premier research finding of biometry in the area of heredity.

So far, there has been no satisfactory historical account of the 'rise and fall' of this 'law', a state of affairs which I hope to remedy in this chapter. I shall examine the fate of this piece of scientific work at the hands of four workers - Galton, Pearson, Weldon and Fisher.

Galton's work

We have seen that Galton was the first to formulate what was to be named, by others, the 'law of ancestral heredity'. As we have seen, it was the product of a spurious derivation from data collected upon the inheritance of height among the English middle classes - that is to say, data on the distribution of continuous variation. The law, as derived, stated that ancestral contributions of different degrees of ancestry in the direct line went as $1/2, 1/4, 1/8$ etc. That is to say, it claimed that we might best predict an individual's height by adding to the mean height of his generation a composite deviate formed by compounding $1/2$ the deviate of his midparent, $1/4$ the deviate of his midgrandparent, $1/8$ the deviate of his midgreatgrandparent, and so on.¹

The law, as employed, was applied to discontinuous attributes (e.g., eye-colour in man and coat-colour in hounds). In this case it was interpreted to mean that the composition of a family would be best predicted by assuming that $1/2$ of the array would follow the parents (i.e., $1/4$ for each parent), that $1/4$ would follow the grandparents (i.e., $1/16$ for each grandparent) and so on, in a manner analogous to the continuous case? Galton's law, therefore, had a number of puzzling features (none of which seems to have stirred up criticism). We have seen already that its derivation was invalid and that it was mysteriously applied to discontinuous situations, though derived in a continuous context. I must now add that the law was given a realistic interpretation by Galton, but a rather vague one. We are told,

for example³, in the penultimate chapter of Natural Inheritance (1889) that the 'personal heritage from either parent is one quarter, therefore, as the total heritage is one half, it follows that the latent elements must follow the same law of inheritance as the personal ones.' The elements referred to are never very precisely described, but judging from other passages in Galton's works, there can be no doubt that he thought of them as real entities whose unseen behaviour governed the observable characters of human (and indeed all) organisms.

Pearson's developments

(a) Introduction

Pearson seems first to have encountered Galton's statistical work when he read Natural inheritance preparatory to giving his talk on Galton to the Men and Women's Club in March 1889⁴. In his discourse, Pearson asserted that long-continued in-breeding among gifted stocks would eradicate the tendency to regression to the mean found by Galton in the population at large⁵ (or, at least, the middle class population at large). Shortly we shall see that it was this desire to show that regression could indeed be overcome by long good-breeding that drove Pearson to conduct investigations into the law of ancestral heredity.

Accordingly, he was anxious to show that there were scientific grounds for his belief that regression could be overcome by good-breeding, and, as we shall see, this desire was to be the motivating force for his work on the theory of multiple correlation (as already mentioned in chapter 4) and, thereby, the development of his distinctive, and entirely non-Galtonian version of what he christened 'Galton's law of ancestral heredity'. His motivation is clearly displayed in the extract from his 1889 talk on 'The laws of inheritance according to Galton' reproduced below.

The general conclusion one must be forced to by accepting Galton's theories is the imperative importance of humans doing for themselves what they do for cattle, if they wish to raise the mediocrity of their race. Wise sexual selection would soon divide any nation into two parts, for either of which Galton's theory might hold, but not for both taken together....I am not advocating a return to group or even close intermarrying, but a far more careful sexual selection on the part of those members of the community who have a large deviation physically or intellectually from mediocrity, ought in itself to tend to differentiate the community and upset satisfactorily the laws of heredity which Galton has based on chance, or what is practically promiscuity of sex.

It seems clear that he thought that a long period of continued good-breeding by an elite could eliminate the tendency to regression based, as he put it, on Galton's assumption of 'promiscuity of sex' - i.e. data mixing long-selected and non-selected lines.

Pearson appears to have been aware of the faults inherent in Galton's law from the beginning of his researches into heredity. He set to work to modify the law and to present it in a defensible fashion, concentrating, no doubt because of his interests in human evolutionary questions, on ¹⁰₂ metrical characters.

In this investigation, his main tool was the multinormal probability distribution. Just as Galton had noted that two generations of human height could be well described by a binormal surface of probability, so Pearson assumed that n generations of a particular human dimension (e.g., height or intelligence) could have its joint distribution adequately modelled by an n -normal probability distribution: namely ⁶

$$(2\pi)^{-\frac{1}{2}n} R^{-\frac{1}{2}} \exp. \left(-\frac{1}{2R} \sum_{i,j} R_{ij} \frac{x_i}{\sigma_i} \frac{x_j}{\sigma_j} \right)$$

with $(i,j, = 1,2,3,\dots,n)$ $R = |(\rho_{ij})|$; $\rho_{ij} = \rho_{ji}$; $\rho_{ii} = 1$

where R_{ij} is the (i,j) cofactor of R .

The consequence of assuming this distribution which is of crucial import to biometry is that, if the deviation from the means of $n-1$ variates X_2, X_3, X_4 etc. assume the values x_2, x_3, x_4 etc., then the distribution of X_1 will be normal about a mean (measured from the general, unconditional mean of X_1)

$$E(X_1 | x_2, x_3, x_4, \dots, x_n) = -\sigma_1 \sum_{j=2}^{j=n} \frac{R_{nj}}{R_{11}} \frac{x_j}{\sigma_j}$$

with variance $\sigma_1^2 R/R_{11}$

Typically, Pearson might interpret the variates X_1, X_2, X_3 etc., to stand for the values of some dimension (e.g., the height or intelligence) in offspring, midparents, midgrandparents, midgrandparents and so on. Accordingly, he readily accepted the conditional expected offspring mean as a predictor of offspring value, given values of midparents $x_2, x_3, x_4, \dots, x_1$

(b) Events of 1895

Pearson's first move in this direction is to be found in his seminal⁷ 1895 paper 'Regression, heredity and panmixia'. Like Galton, he assumed that correlation coefficients could be simply multiplied together to yield values for other correlations; supposing, e.g., that the correlation of an uncle and his nephew might be obtained by multiplying together the correlation values for a father and a son and for two brothers. In particular, he assumed that ancestral correlations would proceed as ρ , ρ^2 , ρ^3 etc.. But, when the correlations take these values, the regression equation (ii) reduced to

$$-\sigma_1 \sum_{j=2}^{j=n} \frac{R_{nj}}{R_{11}} \frac{x_j}{\sigma_j} = \rho_{12} \frac{\sigma_1}{\sigma_2} x_2$$

which, to Pearson, suggested that 'an exceptional father is as likely to have exceptional children if he comes of mediocre stock as if he comes from exceptional stock'. This he found a surprising result, as it in effect denied the existence of ancestral (other than parental) influences. This, of course, went against his earlier expectation that interbreeding among the gifted, pursued over several generations, would nullify the tendency to regression found by Galton in the general population. In 1895, he had to admit that, while the result was a surprise, 'I cannot see how it is to be escaped so long as we assume the normal distribution of frequency, which

appears in so many cases to be a close approximation to fact'.⁸

The surprise lay not simply in the apparent incorrectness of the assumption of ancestral contributions, but also in the evolutionary consequences of this incorrectness. It seemed to follow that, if a population were to be 'improved' by selection, then there would be a reduction of the 'improvement' with each generation - for, each successive generation would have only the fraction $\frac{\sigma_1}{\sigma_2}$ of the 'improvement' shown by the preceding generation.

There were ways out of this awkward consequence.⁹ Pearson considered the possibilities that evolution might operate through either smooth or discontinuous jumps in the 'focus of regression', that is to say, through jumps in the real value from which the deviation $\frac{\sigma_1}{\sigma_2}x$ would be measured. However, despite his suggestion that the 'determination of the focus of regression for some organ in selected domestic ducks for several generations and comparisons with the means for wild and general domestic ducks would seem a possibility', he did not resort to experimentation, but instead decided to question the Galtonian assumption that correlation coefficients could be multiplied together.¹⁰ As a result he was able, in his 1898 'new year's gift' to Galton, to attack the problem of refurbishing the law of ancestral heredity in a new and more promising fashion.¹¹

(c) 1898 and a 'new year's gift'

Introducing the reforms of 1898, Pearson related that the occasion, though not the cause of his doubting the principle of multiplicability of correlation coefficients was Galton's recent successful application of his ancestral law to data for the (discontinuous) coat-colours of hounds. Furthermore, wrote Pearson,¹²

After some correspondence with Mr Galton and an endeavour on my part to represent his views in my own language, I have come to the conclusion that what I shall in future call Galton's law of ancestral heredity, if properly interpreted, reconciles the discrepancies in 'Natural Inheritance' and between it and my memoir of 1895. It indeed enables us to predict a priori the values of all the correlation coefficients of heredity, and forms, I venture to think, the fundamental principle of heredity from which all numerical data of inheritance can in future be deduced, at any rate, to a first approximation.

The paper was mathematically complex, but conceptually straightforward. Pearson interpreted the variates X_1, X_2, X_3 etc. as standing for the values

of some metrical character in offspring, midparents, midgrandparents, and so on. Then, assuming that (i) ancestral correlations diminish in a geometrical series, (ii), that mating was random, and (iii) that the considered character was uncorrelated with fertility, he went on to investigate the truth conditions (under these assumptions) for two forms of Galton's law: a strong form, which may be written,

$$\frac{\sigma_1}{\sigma_j} \sum_{j=2}^{j=n} \frac{1}{2^{(j-1)}} x_j = E(X_1 | x_2, x_3, \dots, x_n) \quad \dots (iii)$$

and a weak form, which may be written

$$\frac{\sigma_1}{\sigma_j} \sum_{j=2}^{j=n} \gamma \beta^{(j-1)} x_j = E(X_1 | x_2, x_3, \dots, x_n) \quad \dots (iv)$$

where

$$\sum_{j=2}^{j=\infty} \gamma \beta^{(j-1)} = I$$

Equation (iii) is just Galton's own law in sophisticated form. Equation (iv) is a more general form, where it is no longer assumed that the coefficients conform to just a single series (1/2, 1/4, 1/8 etc.), but that they conform to some series which is such that;¹³

If an individual had midparents of the same deviation from racial type right away back, i.e., if $H_1 = H_2 = \dots = H_{10} = \dots = H$, we should reasonably expect him also to have a deviation H .

The truth conditions for (iii) and (iv) were discovered to be as follows. The strong form of Galton's law required that midancestral correlations should run as 0.3, 0.15, 0.075 etc.. The weak form had parallel implications, summarised in the table below. ($\gamma = 1$ is, of course, the strong form of the law).

Table of Heredity for diverse Values of γ .

Value of γ .	0.7.	0.9.	1.	1.2.	2.35.	∞ .
Parental correlation	0.2495	0.2851	0.3060	0.3244	0.4000	0.5000
Grandparental correlation ...	0.1243	0.1423	0.1500	0.1624	0.2000	0.2500
Great grandparental correlation	0.0621	0.0713	0.0750	0.0812	0.1000	0.1250
Fraternat correlation ...	0.2499	0.3665	0.4000	0.4536	0.6598	1.0000
Regression on with mid-parent Correlation with with midparent	0.4970 0.4970(0.7071) ^a	0.6701 0.6701(0.7071) ^a	0.8000 0.6000(0.7071) ^a	0.6197 0.6497(0.7071) ^a	0.7999 0.7999(0.7071) ^a	1.0000 (0.7071) ^a

From Proc. Roy. Soc., vol. 66, 1900, p. 149.

(d) A new century and a new interpretation

In 1898 Pearson was unwilling to assert the universal truth of Galton's form of the law (i.e., eqn. (iii)). He noted that the value of 0.3 for parental correlation which Galton's form of the law indicated was, ceteris paribus, harmonious with the value extracted from observational data. But, on balance, he concluded that more data were needed in order to decide 'how far Mr Galton's law needs to be modified.' He thought it 'a priori improbable' that (iii) would cover all cases of the inheritance of normally distributed characters, for¹⁴

I should imagine that greater or lesser inheritance of ancestral qualities might be a distinct advantage or disadvantage, and we should expect inheritance to be subject to the principle of evolution. This difficulty would be to some extent met by introducing a coefficient γ which I propose to call the coefficient of heredity, and consider as capable of being modified with regard to both character and race. As such a law would cover Mr Galton's case, there does not seem any objection to using the more general formula, until it is found that the strength of heredity is the same for all characters and races. Of course, it may well be argued that heredity is something prior to evolution, and not determined by it. If this be so, its absolute fixity for all organs and races ought to be capable of observational proof.

However, by 1900, Pearson appears to have changed his mind. For, in the Grammar of science (his major work on methodology) which was reissued in that year with new chapters on evolutionary biology, he argued that there was now supporting evidence for the conclusion that¹⁵

whenever the sexes are equipotent, blend their characters and mate pangamously, all characters will be inherited at the same rate. Such a result could hardly be attained if evolution itself had produced heredity. It suggests that heredity, like variation, is something fundamental to the vital unit, and is not a product of evolution itself. Environment, largely influencing organs and characters, may fictitiously reduce or increase heredity, if the offspring be not reared in the same environment as their parents; homogamy and other forms of sexual selection sensibly alter the pangamic values of the correlation coefficients; but these modifications of heredity are only apparent, and provide no ground for the assertion that heredity is the product of evolution itself.

Since Pearson makes it clear that the rate referred to is that connoted by taking $\gamma = 1$, it appears that, by 1900, he was an advocate of the position that, ceteris paribus, all continuously varying characters would be inherited according to Galton's own form of the law, namely

$$E(X_1 | x_2, x_3, \dots, x_n) = \sum_{j=2}^n \frac{1}{(j-1)} \frac{\sigma_1}{\sigma_j} x_j$$

This is a remarkable shift. It is a shift from a fairly weak claim - namely that all characters are inherited according to the law with some value of γ , to the extremely strong position that all characters follow just one version of the law (i.e., that which is implied by taking $\gamma = 1$). Together with this claim goes the assertion that, whenever this is not the case, we shall be able to find the cause for the apparent discrepancy in the form of divergences from certain stated conditions - e.g., divergencies from random mating. Even more interestingly, it is now claimed that 'heredity, like variation, is something fundamental to the vital unit, and is not a product of evolution itself.'

Before discussing the significance of these remarks, a brief review of the logical structure of the law of ancestral heredity and of its empirical content is in order. Let us commence with an attempt to understand precisely what it was that the law asserted, focusing on its 'strong' form - i.e., the form given by equation (iii). First of all, let us see how it did appear to overcome the problem posed to Pearson by Galton's observation of a tendency to regression observed among the population generally.¹⁶

Let us consider the case where, starting with the exceptional offspring of unexceptional parents, one selected (in the sense of allowed to breed) for n generations, only those individuals which showed a deviation of k units from their generation's mean. Assuming that all ancestry before the onset of selection was mediocre, and that the variance of each generation was the same, it followed, by Galton's law, that the offspring in the $(n+1)$ th. generation would have a deviate of,

$$K \left(\frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \dots + \frac{1}{2^n} \right) = K \left(1 - \frac{1}{2^n} \right) \text{ units}$$

If selection was then relaxed, and in-breeding commenced, the next generation would have a deviate of,

$$K \left(\frac{1}{2} \left\{ 1 - \frac{1}{2^n} \right\} + \frac{1}{4} + \frac{1}{8} + \dots + \frac{1}{2^{n+1}} \right) = K \left(1 - \frac{1}{2^n} \right) \text{ units}$$

and the one after a deviate of,

$$K \left(\frac{1}{2} \left\{ 1 - \frac{1}{2^n} \right\} + \frac{1}{4} \left\{ 1 - \frac{1}{2^n} \right\} + \frac{1}{8} + \dots + \frac{1}{2^{n+2}} \right) = K \left(1 - \frac{1}{2^n} \right) \text{ units.}$$

and so on.

The offspring would always show the same deviate as had the generation before selection was ceased. There was, it seemed, no need to suppose, as had Galton, that new, stable races could arise only when a sport - an exceptional individual whose posterity regressed to a new racial mean - was produced by variation.

There are difficulties associated with this result. The first is connected with the notion of characters measured as deviates from the mean character for their own generation. It seems that in order to make any predictions about the outcome of a process of selective breeding - cases where, by definition, there would be no general population of offspring, but only the offspring of selected parents - one would have to assume that, if there had been no selection, the population would have gone on reproducing itself with the same mean from one generation to the next. Otherwise it would be impossible to decide whether an individual one intended to breed from exhibited a deviation of K or not. All that one would know would be the absolute value of the character in question.

The second difficulty concerns the range of applicability of the law. Pearson's version of Galton's law predicted that the standard deviation of all arrays would be equal to $\sigma_1 \sqrt{1-R^2}$, or to about 89% of the standard deviation (σ_1) of the offspring generally. Within these arrays there should be forms more extreme than their parents, which, in theory, could be bred from, and so on - leading automatically to the emergence of more and more deviate forms.

Was this really the case? Or had Darwin's critic Fleeming Jenkin been correct when he had asserted that, after a few generations of selection,¹⁷ 'no further perceptible change can be effected'? As far as I have been able to ascertain, the biometricians did not discuss this crucial problem in print - though Weldon, we shall see, did have thoughts on the matter.

(e) Homotyposis

Returning to the main line of the narrative, we must ask why it is that Pearson's views so increased in boldness in, or about, 1900. It seems clear that the change is intimately bound up with the development of his theory of homotyposis, which he related that he was engaged upon during the summer of 1899.¹⁸ This theory was a very complex work, but, for present purposes, a brief outline of salient points will suffice.¹⁹

Here, the notion of the 'undifferentiated like organs' put forward by an individual plays an important role, and, as a paradigm, Pearson had in mind the leaves of a particular tree, which are continuous in their dimensions and which are not differentiated according to function. The notion is intuitively understandable (red blood corpuscles, and scales from a moth's wing are other examples), though, as Pearson's rival, William Bateson was to suggest (in a rather forceful manner), perhaps not entirely clear and distinct.²⁰

Pearson was impressed by his observation that the variability (standard deviation) of the undifferentiated like organs put forward by an individual, when measured with respect to any dimension (e.g., leaf breadth) was generally between 80 and 90% of the variability of the dimension in the species as a whole. This seems to have been the origin of his belief that variation was in some way fundamental to the vital unit. His Grammar of science indicates how his thought advanced from this observation to the consideration of gametes as examples of undifferentiated ^klike organs, and then to the investigation of what this implied.²¹ ^

Now, if we consider sexual reproduction, we find the male individual producing a number of male reproductive cells, the male gametes, and the female individual a number of somewhat different reproductive cells, the female gametes. Each individual gives a group of gametes of a given individual type and given individual variability. The conjunction of two gametes, male and female gives what has been termed the zygote or stirp, the origin of a new individual....A group of offspring from the same parents are not all alike, because the conjugating gametes are taken, let us assume for the present at random, from two groups, all members in either of which are not alike. The variability among brethren is thus seen as a direct corollary to the law according to which any individual puts forth a group of undifferentiated like organs. The investigation of the relation between the law of individual growth and the variability of brethren is too complex to be given here, but the point to be insisted upon is this: the resemblance between brethren, or, indeed, any pair of relatives, is a consequence of the resemblance, that is the degree of correlation, between undifferentiated like organs in the individual. Allow for environment, allow for growth, and yet the parts of an individual are not identical. What is the bathmic influence which produces this variability? We can demonstrate the existence of this variability, we can describe it quantitatively, but the why of it is as much a mystery as the why of the law of gravitation.

This line of thinking was much developed in Pearson's 1901 paper on the theory of homotyposis, where he was led to predict that the degree of correlation between undifferentiated like organs in the individual was responsible for, and would be equal to ~~the degree of correlation in the individual was responsible for, and would be~~ equal to the degree of correlation between brothers. Empirical investigation gave substance to this expectation, for, in 1900, he was able to write:²²

Is the correlation between pairs of undifferentiated like organs in the individual the same or nearly the same for all forms of life? If so we have ascertained quantitatively as comprehensive a law of growth for living organisms as the law of gravity for molar masses. My researches on this point are not yet complete, but they indicate that the following law is true. The degree of resemblance between undifferentiated like organs in the individual is nearly the same for all forms of life, and its mean lies between 0.4 and 0.5. We shall speak of this result as the law of growth of like parts.

and, in 1901, he was able to claim that²³

The mean of twenty-two homotypic series is found to be sensibly identical with the mean of nineteen fraternal series. (Note that the correlation between the undifferentiated like organs of the individual was referred to by Pearson as 'homotypic correlation')

which, given that there was no relation between the 'relative simplicity of the organism and the intensity either of its variability or its homotypic correlation', led him to believe that²⁴

there seems ^{some} ground for supposing that homotyposis (and therefore heredity) is a primary factor of living forms, a condition for the evolution of life by natural selection, and not a product of such selection. If the mushroom, the beech and the poppy show approximately equal homotyposis, it seems well nigh impossible to consider it as a factor of life, increasing with advancing evolution.

In other words, it would seem that, by 1900, Pearson believed that he had found a fundamental and innate tendency of living organisms; namely the tendency to produce undifferentiated like organs which had a high variability and which obeyed a 'law of growth' to yield homotypic correlations of about 0.4. Sperms and ova, he suggested, were also examples of undifferentiated like organs, and, via the theory of homotyposis, was

led to expect that the homotypic correlation between the gametes of the individual was responsible for, and equal to the correlation between brothers. This equality was empirically confirmed, and values of about 0.4 were found for both coefficients in a wide range of types of data. Since it was Galton's law with $\gamma = 1$ which predicted a fraternal correlation of this value, Pearson came to suppose that it was this version of the law which, ceteris paribus, governed the inheritance of all continuously varying characters. As such it was a principle of truly 'Newtonian' importance.²⁵ Galton's law now took the centre of the stage as the expression of the consequences of one of the most basic, innate and mysterious of biological processes. Here of course we have the biological version of Pearson's generalised statistical cosmology. Unfortunately, taking the centre of the stage may lead to embarrassment, and thence to a tactical retreat. So it was with Pearson's contention that all characters were inherited according to Galton's law with $\gamma = 1$.

(f) Modifications of 1903

By 1903, he had observed sets of ancestral correlations which,²⁶ though conforming to a geometric series, did not conform to one which connoted either the coefficient series $1/2, 1/4, 1/8$ etc., or, indeed, any coefficient series $\gamma\beta, \gamma\beta^2, \gamma\beta^3$ etc. which summed to unity. With the abandonment of both the strong and the weak form of Galton's law as universal truth (Pearson did not decide to retain either form by denying the ceteris paribus clause built into the law) went the abandonment of the expectation that the relaxation of selection, and the commencement of in-breeding would yield population stability. In the cases studied (eye-colour in man and coat-colour in horses) the correlation values were such as to lead to the expectation that 'after selection ceases a very slow regression sets in, which would be hardly perceptible without very definite quantitative measurements for the first three or four generations of inbreeding'.²⁷

More generally, by 1903, the whole face of the law of ancestral heredity was much changed. Pearson now made it clear that he no longer held that he had discovered 'the single statement which embraces the whole field of heredity', and which must prove 'almost as epoch making to the biologist as the law of gravitation to the astronomer'. Instead, he

now emphasised that there was no real 'law' at all, but, rather, that there were many benefits to be gained from using the mathematics of multiple correlation to make predictions in the sphere of hereditary data. Thus we find him emphasising that:²⁸

The law of ancestral heredity in its most general form is not a biological hypothesis at all, it is simply the statement of a fundamental theorem in the statistical theory of multiple correlation applied to a particular type of statistics. If statistics of heredity are themselves sound, the results deduced from this theorem will remain true whatever biological theory of heredity be propounded.

and that;

The law of ancestral heredity as founded on the theory of multiple correlation involves no biological theory of regression. The term regression has unfortunately been taken from statistical theory and interpreted in a biological sense. In statistics the regression is always to the mean of the foreknown character. Further, if there be a number of cognates, we can a priori, i.e., before quantitative analysis, not state the total amounts they will contribute to the predicate will or will not indicate a biological regression.

Again, in 1909,²⁹ Pearson offered a similar, but now more explicit epitomisation, stating that 'the law of ancestral heredity is embraced in the following statements':

- (i) In a population breeding without assortative mating the regression line for offspring on any ancestor is linear.
- (ii) The correlations between offspring and successive grades of ancestry form a progression diminishing geometrically as we ascend to distant grades; and
- (iii) The general relation of an individual to his ancestry can be closely expressed by the multiple correlation formula.

This appears to have been his final stance on the subject, publicly defended as late on as 1930.

So far then, it would seem that the law of ancestral heredity went through an amazing series of developments. Galton never referred to his formula by this name, which was coined by Pearson, who, by turn, seems to have varied its designation with high frequency. The results so far may be expressed in tabular form:

Interpretation	author	reference	coefficients
1st (1889)	Galton	continuous + discontinuous characters, Also genetic particles.	$\frac{1}{2}, \frac{1}{4}, \frac{1}{8}$ etc.
2nd (1895)	Pearson	continuous characters. No discussion of genetic particles.	no 'ancestral' coefficients at all.
3rd (1898)	Pearson	continuous characters. No discussion of genetic particles.	Any series where $\sum \gamma \beta = I$ various values in different cases.
4th (1900)	Pearson	continuous characters. Reference to basic processes of homotyposis.	$\frac{1}{2}, \frac{1}{4}, \frac{1}{8}$ etc.
5th (1903 etc)	Pearson	continuous characters. No reference to genetic processes.	"Best" geometric series of coefficients that can be fitted to the data.

This might be thought to be the final word, but we still have two further interpreters of the 'law of ancestral heredity' to consider, namely W.F. R. Weldon and Sir Ronald Fisher.

W.F.R. Weldon

Weldon, we have seen, was engaged upon the construction of a physiological theory of heredity along Galtonian lines in the early years of the 20th century, at a time when it seems that Pearson was rather discounting any biological interpretation for the 'law of ancestral heredity'. Weldon's notes³¹ contain the following revealing passage, which, judging by context, seems to serve both as an exposition of Galton's views and as a statement of sympathy thereunto. What is particularly interesting is the way in which Weldon seems to have assumed that Pearson, like himself, interpreted the coefficients of the law of ancestral heredity as referring to complements of genetic particles.

"In all these cases he found that the facts of inheritance could be expressed by supposing that the dominant elements, which are distributed among the individuals of a generation and determine their visible characters, are derived from the dominant determinants of parents, grand-parents and more remote ancestors, in proportion to the terms of a geometrical series whose sum, representing the total heritage of the generation, is unity."

"The series originally proposed to represent the proportions in which the ancestors of different degrees of remoteness contribute to the characters of a generation is the series

$$\frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \dots \text{etc.,}$$

the parents together contributing one half the total heritage, the grand-parents one quarter, and so on. This series was given only as a first approximation to the result of observations which were new and not sufficiently extensive to give accurate values for the various terms.

The subsequent work of Pearson and his pupils makes it necessary to change the actual values, though it only confirms the statement that the form of the relation between the characters of one generation and those of its ancestors is closely given by coefficients of correlation and regression which form a geometrical series, the terms of which are nearly the same

for a number of characters in Man and in several of the lower animals.

The form of the series thus established is the essential point of Galton's work, and is all we need attend to for the moment, leaving the question of the actual values of the terms for description later."³²

This general line of thinking was also proposed in Weldon's lecture³² of 1905 on 'Inheritance in animals and plants', where he discussed mechanical models for the production of correlations. I will not discuss these here as it is a matter to which we will return in the chapter on psychology. Rather more interesting, perhaps, in any case, is the line that Weldon's thoughts seem to have taken on the topic of the limits to variation. We have seen, in an earlier section, that the biometricians had surprisingly little to say, in public at least, on the matter of the amount of change which they expected might be produced by the continued selection of organisms in the 'tail' of a normal distribution. The mathematical model which they adopted, if taken seriously, seemed to indicate that, in theory at any rate, the, say, tallest members of a population, ^{when} interbred, would always produce even taller children, who could be bred to produce yet taller individuals - and so on. This, of course, was a point which opponents of Darwinism had long denied. Since at least the date of Fleeming Jenkin's review of the fourth edition of the Origin of species, Darwin himself, evidently, had been discomfited by this point, and was able to respond in a manner that seemed to pin hopes on what might happen after a very long period had elapsed.³³

In order that any great amount of modification in any part should be effected, a variety when once formed must again, perhaps after a long interval of time, vary or present individual differences of the same favourable nature, and these must again be preserved, and so onwards step by step. Seeing that individual differences of all kinds perpetually recur, this can hardly be considered as an unwarrantable assumption.

Weldon, it seems, was attracted by the idea of constantly modifying genetic particles. This was a mechanism which would enable him to overcome Jenkin's objection, though, of course, it offered, in itself, no very precise answer to the question of the natural limits to which a population might be predictably changed by continued selection. We see in the extract below, that Weldon, like Pearson, seems to have favoured a doctrine of innate variability to one of homogeneous classes, punctuated by occasional

discontinuities.³⁴

'If the determinant elements, which this view of the mechanism of transmission postulates, are supposed to pass from one generation to another without any other variation except that involved in the assumption of the dominant or the latent condition, then a very important limit to the range of possible variations is introduced, because the only characters which can be supposed to appear among the individuals of a generation are those produced by mixing the characters of the ancestors in various proportions; any really new character can only appear by the occurrence of some disturbance which either introduces new determinants or leads to an abnormal change in the characters of those already present, so that the appearance of a new character must on such a view be ascribed to a process other than that of normal inheritance, leading to a "mutation" or "sport". If, however, the characters of each determinant may be assumed to vary to any extent, however slight, during its transmission from one generation to another, a gradual production of new characters .. may conceivably result from a normal process of inheritance, and the necessity for invoking a new process to account for their appearance will not arise.' (emphasis added)

R.A. Fisher

We have seen that Pearson appears finally to have steered away from giving his formalism any biological interpretation. Even in 1930, in a long paper on ³⁵'A new theory of progressive evolution', published in the Annals of eugenics, he was to be found asserting, quite correctly, that 'the law does not depend upon any mechanism of the germ plasm'. But, in the meantime, the world had not stood still, and R.A. Fisher - the future Sir Ronald Fisher - had been at work.³⁶ We shall return to him and his work in the chapter on eugenics - but, for the moment, it is worth noting that in his famous paper of 1918, "On the correlation of relatives on the supposition of Mendelian inheritance", he showed that observed correlations between relatives could³⁷ be explained in a Mendelian fashion. Now, as Pearson noted all along, the coefficients of the ancestral law (e.g., $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$ etc.) - which governed mid-ancestral deviates - were functionally related to the simple correlation coefficients linking father and son, grandfather and grandson, and so on. Fisher, therefore, was able to show that the law of ancestral heredity was a consequence of Mendelian genetics - or, at least, that a law of that ^{general} form was. To this matter we shall return in Chapter 8.

- 1 See for discussion, my chapter 2.
- 2 Galton applied his law to discontinuous attributes as soon as he had formulated it - see for example, chapter 6 of his Natural inheritance, London (1889), which discusses data for the inheritance of eye-colour. See also his later paper, 'The average contribution of each several ancestor to the total heritage of the offspring', Roy.Soc.Proc. , 71 (1897), 401 - 413. A 'standard' paper on Galton's law, namely R.Swinburne's 'Galton's law - formulation and development', Annals of science , 21 (1965), 15 - 31, does not appear to discuss this anomaly.
- 3 F.Galton, Natural inheritance, London (1889), 188.
- 4 This has been discussed in chapter 4 of this thesis.
- 5 As recounted in chapter 2, Galton thought that the existence of regression implied that the effects of selection could not be maintained when selection was relaxed.
- 6 This was outlined in Pearson, op.cit. (note 7). To simplify matters I have used here not Pearson's notation, but that of Hilary Seal in his 'Studies in the history of probability and statistics.xv. The historical development of the Gauss linear model', Biometrika, 54, 1 - 24.
- 7 K.Pearson, 'Mathematical contributions to the theory of evolution.3. Regression, heredity and panmixia', Phil.trans.Roy.Soc. , 187A (1896) 253 - 318.
- 8 Ibid. ,306.
- 9 Ibid.., 308 - 317
- 10 Ibid. ,276.
- 11 K.Pearson, 'Mathematical contributions to the theory of evolution. On the law of ancestral heredity', Proc.Roy.Soc..,62 (1898), 386 - 412.
- 12 Ibid., 386.
- 13 K.Pearson, The grammar of science, 2nd. edn (London,(1900), 478.
- 14 Pearson, op.cit. (note 11), 411 -412
- 15 Pearson, op.cit. (note 13), 480
- 16 This was discussed in Pearson, op.cit. (note 11).
- 17 See Fleeming Jenkin, 'The origin of species', North British Review, 46 (1867), 277 - 318. For an expanded discussion of this claim, see P.Vozzimer, Charles Darwin, the years of controversy , London (197
- 18 This is revealed in K.Pearson, W.F.R.Weldon, '1860 - 1906', Biometrika , 5 (1906), 1 - 52.
- 19 The main locus of Pearson's work on homotyposis is K.Pearson, , 'Mathematical contributions to the theory of evolution.9. On the principle of homotyposis and its relation to heredity, to the variability of the individual and to that of the race. Part 1. Homotyposis in the vegetable kingdom', Phil.trans.Roy.Soc.., 187A (1901), 285 - 379.

- 21 Pearson, op.cit. (note 13), 450 - 451.
- 22 Ibid., 403
- 23 Pearson, op.cit. (note 19), 363.
- 24 Ibid.
- 25 In op.cit. (note 11), Pearson had spoken of his revamped version of Galton's law as possibly proving 'almost as epoch - making to the biologist as the law of gravitation to the astronomer'. See his p.412.
- 26 K.Pearson, 'The law of ancestral heredity', Biometrika, 2 (1903), 211 - 229.
- 27 Ibid As far as I can see, Pearson never thereafter . was able to explain how the selection of continuously varying characters could lead to stable results, except, if by chance, the relevant coefficients should sum in the manner he had hoped for in and before 1990.
- 28 Ibid
- 29 K.Pearson, 'The theory of ancestral correlations in heredity', Proc.Roy.Soc., 81B (1909), 219 - 224. See p.219
- 30 K.Pearson, 'On a new theory of progressive evolution', Annals of eugenics, 4 (1930), 1 - 40.
- 31 These have been referred to in chapter 5. See note 96, chapter 5.
- 32 W.F.R.Weldon, 'Inheritance in animals and plants', in ed. T.B.Strong Lectures on the method of science , Oxford (1906), 81 - 109.
- 33 Charles Darwin, Origin of species, 5th.edn ,London (1869),Chapter 4.
- 34 Weldon, op.cit. (note 31)
- 35 Pearson, op.cit. (note 30)
- 36 See the discussion of Fisher in chapter 8.
- 37 See R.A.Fisher, 'On the correlation of relatives on the supposition of Mendelian inheritance', Transactions of the Royal Society of Edinburgh, 52 (1918), 399 - 433. For Fisher's explication of the ancestral law, see pp. 421 - 422.

Chapter 7. Eugenics as a popular movement, would-be science
and midwife of science: the case of R.A.Fisher.

Introduction

In several of the six preceeding chapters the topic of eugenics has appeared. We have seen for example that eugenic ideology was an important factor in the thinking of the pioneers of biometry and statistics. This chapter is devoted entirely to the British eugenics movement in the period prior to 1920, and looks at this movement and its influences under three headings.

I begin by discussing what might be termed the 'popular' side of eugenics - by describing and analysing the tide of eugenic thought that flowed in Britain in those early years. In the second part of the analysis I shall examine the work of the Galton Laboratory under Pearson, who saw his organisation as offering a rational and scientifically sound alternative to the popular movement, an alternative which could supply the nation's governors with a scientifically based set of guides to policy in vital social matters. Finally, I shall turn to the effects of these eugenic institutions upon sciences other than the would-be science of eugenics as developed by Pearson. The areas where the influence was greatest were genetics and psychology, and, as the next chapter is given to psychology, I shall here look at genetics, focusing in particular on the work of that seminal figure Sir Ronald Fisher.

Eugenics as a popular movement.

It is gratifying to note that the British popular eugenics movement has begun to receive the attention of historians. Most notable in the field is G.R. Searle's recent monograph on Eugenics and politics in Britain, 1900-1914.¹ The existence of this literature has the happy effect of making it possible to briefly describe the salient features of the movement, secure in the knowledge that a great deal of detail resides in the works of Searle, and also of Lyndsay Farrall, author of a distinguished doctoral thesis on the English eugenics movement.²

We have already seen that Galton was long a eugenicist, but that he did not take to the public platform much before the turn of the century. Before that turning point, it does not make much sense to speak of an organised eugenics movement in Britain, though, as always, several figures who were 'ahead of their times' can be uncovered. One such is William Rathbone Greg the essayist who, in 1868,³ published on the 'Failure of "natural selection" in the case of man', arguing that society had reached

a state in which the 'unfittest' were surviving. This was a state of affairs which he deplored, and he wrote with feeling of a better society in which 'all candidates for the proud and solemn privilege of continuing an untainted and perfecting race should be subjected to a pass or competitive examination.' Interestingly though, Greg, unlike Galton, was a believer in the inheritance of acquired characters. Consequently, his schemes had not the wholly Calvinistic tinge typical of Galton's ideas, which made so much play of doctrines of the genetically elect. This comes out clearly in the passage from his essay reproduced below - a passage that reveals Greg to have been of views very different from Galton's.

I cannot see why - when the working-classes are educated in some proportion to those now above them, and possess property of their own, - whether in acres, or consols, or shares, as they assuredly may do, and soon will - they should not become so provident and so well-conditioned, that they will be no unfit fathers for coming generations. For we must never forget that it is not poverty, but squalor - not a hard life, but insufficient nutriment - not strenuous bodily exertion, but excessive and exhausting toil - that disqualify men from transmitting a sound physical and mental constitution to their offspring. A sanified city population and a well-fed agricultural population may be not only a wholesome but a necessary element to share the functions of paternity with the more elaborately prudent and cerebrally over-developed classes higher in the social scale.

In this respect, Greg's thought resembled that of other noted late 19th century social commentators - and, indeed, as we shall see, also that of many 20th century eugenicists - commentators such as, for example, the eminent economist Alfred Marshall, who wrote of the London poor in 1884 that⁴

even when their houses are whitewashed, the sky will be dark; devoid of joy they will tend to drink for excitement; they will go on deteriorating; and as to their children, the more of them grow up to manhood, the lower will be the average physique and the average morality of the coming generation.

And, in a similar vein, we may find Charles Booth's collaborator Llewellyn Smith arguing that⁵

It is the result of conditions of life in great towns, and especially in the greatest town of all, that muscular strength and energy gradually get used up; the second generation of Londoner is of a lower physique and has less power of persistent work than the first, and the third generation (where it exists) is of lower than the second..

I have mentioned these two writers as (i) they are good examples of the fear of urban degeneration that existed in late 19th century Britain among middle class social theorists, and (ii), they show that among this fraction of the intellectual classes at any rate, some version of the doctrine of the inheritance of acquired characters was a commonplace. This, as we shall see, opened the possibility of simultaneously blaming all sorts of conditions - poverty, insanity etc. - on 'defective germ plasm' whilst at the same time maintaining that poor environment was the cause of that defect. They could thus require immediate eugenic measures with long-term hopes for environmental reform.

If we turn to leading 19th century British biologists we find no massive enthusiasm for Galton's views. Darwin was aware of Galton's views, and, indeed, once wrote to him a most congratulatory letter on Hereditary genius. He was, in general, in favour of there being a high fertility among the 'fit' section of society, but could not bring himself to support Galton's view that human stock-breeding was the only genuine pathway to human progress. In his Descent of man, for example, he depicted Galtonian 'genetic' progress as less important than the progress brought about by ⁶ 'a good education during youth whilst the brain is impressible', and by 'a high standard of excellence, inculcated by the ablest and best men, embodied in the laws, customs and traditions of the nation, and enforced by public opinion.' Darwin was a much broader man than Galton, it should never be forgotten; ~~he~~ was the least dogmatic of men. The differences between the two may be nicely gauged by comparing Darwin's Voyage of the Beagle with Galton's book on his travels in Tropical South Africa.

Galton met with no more success with Alfred Russell Wallace, whose views on the evolution of the mind make him one of the most fascinating of all the great Victorians. Wallace, in general, was sympathetic to the idea that breeding should be done predominantly by the gifted. But, in his view, this would arise as a consequence of a transition to socialism. In such a system, equality between the sexes would come into play, and in consequence, women would choose not to allow themselves to become impregnated by the lower sorts of men.⁷

I have made these various points because they indicate again the exceptionality of Galton. Hardly anyone in late 19th century England was prepared to join Galton in his 'religious' quest for human progress via

stockbreeding methods. If Galton's eugenic views were to become popular, then they would have to be modified so as to become acceptable to a group with views on society that were a great deal less idiosyncratic than Galton's. This, we shall see, is what happened in the early 20th century; when a new band of Galton's supporters differed from their master in several points.

Galton^{as noted} stepped onto the public platform in 1901⁸ when he gave the Huxley lecture at the Anthropological Institute, speaking on 'the possible improvement of the human breed under existing conditions of law and sentiment'. In this address, Galton made a rough equation between levels of 'civic worth' and social class and stressed the need for higher levels of breeding from those classes judged to be of higher civic worth. Repeat performances, so to speak, were then given to the Sociological Society in 1904 and 1905.⁹ There is little point in analysing these talks, as they all carried roughly the same message - namely the message of possible human progress (or, at least of Anglo-Saxon progress) brought about by a two-pronged strategy. One prong, sometimes called that of 'positive eugenics', advocated measures to raise the fertility of the classes of greatest civic worth. The other, sometimes called 'negative eugenics', advocated measures to stem the fertility of the classes of lowest civic worth. Galton, in general, was much more enthusiastic about positive than about negative eugenics. His philosophy, from the beginning, was not one that focused upon the 'problem' of the supposed proliferation of the unfit.¹⁰ It was a distinctly utopian philosophy which looked forward to painless progress. We have seen already that Galton backed his hopes with his purse, setting up the Eugenics Record Office in 1904, and leaving money to install Pearson as professor of eugenics in 1911.

As Galton's long life reached its conclusion, it seemed that his views were at last beginning to be taken very seriously, for in November 1907, a meeting of members of a 'Moral Education League' and of others so interested in Caxton Hall,¹¹ led to the setting up of a 'Eugenics Education Society'. This still survives, though under the shorter designation of 'Eugenics Society.' I have not found out much of interest about the Moral Education League, but point out that Edwardian England was a considerable place for the emergence of societies given to the protection of other people's morals:

Those who have read Edward Hynes' Edwardian turn of mind will find nothing out of place in the existence of such an institution. Dr J.W. Slaughter was elected provisional chairman, and Mrs Gotto, a lady of force and charm, was elected secretary. In early 1908 the first annual general meeting was held, and shortly thereafter, the society gained instant publicity by becoming engaged in a campaign to stop the London County Council from closing certain hostels, thereby turning numbers of 'inebriate women' onto the pavement and, doubtless, onto the streets too. In 1910, a journal, the Eugenics review, was launched, and was able, shortly afterwards, to report the proceedings of the first ever International Eugenics Conference, held in 1912, in the University of London, with Major Leonard Darwin presiding.¹³ Recruitment to the Society was brisk, and, as Searle notes, 'by 1914, membership had risen to 634, and there were affiliated branches in Belfast, Birmingham, Haslemere, Liverpool and Oxford, as well as other discussion groups, as in Brighton'.¹⁴

The membership of the Society was remarkable on two counts - firstly, it contained a number of distinguished persons, drawn from the professions rather ^{than} from the aristocracy or from the commercial classes. Secondly, the membership contained a very high proportion of women - perhaps unsurprising given that eugenics was, at heart, about giving birth. To illustrate the first point, I have recorded below the names of the office-bearing members for 1913-14.¹⁵ Women were proportionally more numerous among the non-office holding members.

- Image removed due to third party copyright

In the pages of the Society's journal¹⁶ we can trace its concerns. We find articles discussing the birth rate and whether it was producing too many persons of the 'less fit' sort, we find articles on heredity, on the nature-nurture issue, and on the possible hereditary origins of alcoholism, poverty, mental illness and criminality. In practice, these followers of Galton, who persuaded Galton to become honorary president in 1910, were much preoccupied with the ideas of negative eugenics, and with the prospect for negative eugenic solutions to these problems. A fairly typical sentiment was that expressed by the 'Committee appointed to consider the eugenic aspect of poor law reform', when, in 1911, it claimed that 'a great part of pauperism lies outside the operation of normal economic processes', and this because paupers were frequently persons stricken with hereditary degeneracy. The same committee looked forward to the day when state-segregation had led to the extinction of the thousand or so family 'stocks' that made up a hereditary degenerate class.

In practice, the utterances of the Eugenics Education Society seem to have led to almost no concrete results in the form of legislative change. Here, of course, the British scheme differed mightily from the American, where forcible sterilization soon gained a place on the statute books of a number of States, often to the accompaniment of approving words from prominent geneticists.¹⁷ Indiana effected a sterilization law in 1907, and, by 1926, twenty-three states had followed suit. If the eugenics movement did have a finest hour in Britain, then it came in 1913, when there was passed a mental deficiency act, which came about as follows. In 1904, there was set up a Royal Commission into the Care and Control of the Feeble-Minded.¹⁸ Exactly why it was then set up is unclear, though Searle instances the influence of prison and poor-law authorities who were able to discern that considerable fractions of their clientele were not of good mental development.¹⁹ The evidence given by experts suggested strongly that the condition was an hereditary one, and the commissioners suggested that a degree of segregation (to prevent procreation) should enter into the solution of the 'mental deficiency' problem. The mentally defective were also presented as a major problem in the 1909 Reports of the poor-law Commissioners. The minority report, for example, noted that, in 1906, the workhouses of England and Wales contained 11,151 persons 'certified to be of unsound mind', and calculated that²⁰

the total number of mentally defective person
now residing in the ordinary wards of the
General Mixed Workhouses of the United Kingdom
must amount to more than 60,000.

This, the commissioners indicated in no uncertain tones, was scandalous. The various reports gave rise to hopes of legislation, and we know that Winston Churchill was anxious to cease what he saw as the rapid decay of the nation by restricting these persons further and by sterilizing them, possibly with X-rays. Government action, however, was slow to come, and¹⁸ was only after the Eugenics Education Society and the National Association for the After-Care of the Feeble-Minded banded together for lobbying purposes that, in 1912, the government produced its mental deficiency bill which allowed for the setting up of new provision for the segregation of the mentally defective. However, it was not until 1914 that the legislation received the Royal Assent, and, by then it had been gelded by the removal of its 'most eugenic' component, the clause 17, which allowed for the feeble minded to be restrained when it was judged 'desirable in the interests of the community that they should be deprived of the opportunity of procreating children'. But, as Searle remarks²¹, it was nonetheless a piece of legislation that did owe a great deal to 'the eugenists' demands for the curbing of the multiplication of the unfit'. In the event, it would seem, the passage of the Mental Deficiency Act was, in Britain, the high-water mark of eugenic legislation. The first war interrupted the eugenics movement as it did so many other things, and, by 1920, membership had slipped to 494. The consensus among historians is that the eugenics movement weakened steadily after the first war, though there is room left in the classification of the nature of the weakening - whether for example, it was just in membership of the Eugenics Education Society, or in its 'influence' or in both.²²

Here then, we have a brief picture of the popular eugenics movement in Britain prior to 1920. It spent some of its energies in the advocacy of 'positive' eugenic moves, in, say, advocating tax-advantages for the fertile members of the professional middle class, but its main concern seems to have been focused on negative eugenic issues - on crime, on alcoholism and so on, and on the possibility of reducing these by the restriction of 'unfit germ plasm'. Needless to say,²³ Karl Pearson was implacably hostile to the popular side of eugenics, and it is not hard to sympathise with him. For, just at the time when he was attempting to establish eugenics as a

science, based on his new statistical methodology, he was faced with the uprising of a popular movement, which, one could be certain, would be classified by the public mind as the central focus of eugenics. Consequently the fate of eugenics as a science could perhaps be endangered by being so associated. One feels that it was rather like the biometric-Mendelian business all over again. Much as the novelty and authority of biometry had been challenged by the emergence of a Mendelian school, so, now, the new would-be science of eugenics being developed in the Galton Laboratory was being denied its pole position by the emergence of a popular movement. The minute-book of the Cambridge University Eugenics Society²⁴ contains a copy of an undated letter of Pearson, to the Times, in which he wrote with feeling of the way in which eugenics had been turned into 'a subject for buffoonery on the stage and in the cheap press', and went on to complain that

We are treated to 'eugenic' marriages and to 'eugenic' babies and the 'eugenic' plays which have nothing whatever to do with the problem of race-welfare; officials of eugenic societies submit to being interviewed with regard to well-ordered babies, and anyone who stands wholly apart from such absurdities may wake one morning to find his name associated with a 'eugenic' baby, whose very existence he has never heard of.

Eugenics, as he had feared, was rapidly degenerating into a topic for the poseur and 'Kongressbummler', but 'years of patient work in medico-social observation, in genetic experiment, and in careful study of family history', he claimed, were needed before

the laws of eugenics as a science can be dogmatically stated.

Here then, in brief at least, we have the English eugenics movement as it existed in the first twenty years of the century. It does raise, I think, a number of interesting questions for the social historian. On the face of it, we have the phenomenon of Galton acquiring an organised following in this period, which did manage to exert some political influence. Churchill, it seems, described himself as a eugenicist (adopting a Galtonian neologism) and G.K. Chesterton thought it worth writing a book on Eugenics and other evils.²⁵ The movement, it seems, lost somewhat in influence after the first war. We are faced therefore with the questions of why Galton acquired a following only after his 70th birthday, for a cause he had

advocated since 1865, and of why, after flourishing in the period prior to the first war, popular eugenics thereafter went into something of a decline. Then, of course, there is the issue of why it is that British eugenisists were never able to pass the sort of sterilization legislation that appeared in the United States.

In this work, which focuses on the history of science in Britain, I shall leave this last and most interesting question to one side. The other questions I will tackle, but in an incomplete manner - and with the same excuse. What I hope to do in the remainder of this section is to briefly outline the answers to these questions deployed by the historians of British eugenics, and, by criticising them, to show that they are far from satisfactory and to indicate that work still remains for the social historian.

All writers on the eugenics movement appear to agree on at least two propositions, namely (i), that the eugenists were drawn predominantly from the professional middle class, notably from the medical profession, and (ii) that the age of eugenics - i.e., the period from the turn of the century to the outbreak of war - was one suffused by an ideology of 'national efficiency',²⁶ by a feeling shared by Fabian and Capitalist alike that Britain was falling behind Germany in the struggle for world leadership, and that this could be countered only by measures that would strengthen the flagging imperial race. Interestingly, one of the effects of the Boer War reverses was to focus attention not simply on the incompetence of generalship but also onto the possibility of there being an ongoing decline in the standard of men available for recruitment into the ranks.²⁷ The suggestions led to an official inquiry which found that there was no evidence to suggest any such decline, but, it would seem, a common sentiment of the period was one to the effect that the imperial race was declining and that this was due to an ingrowing cancer of fast-breeding degeneracy. Shortly, we shall see how Karl Pearson responded to and fed these fears.

When writers on the eugenics movement have attempted to mobilize these facts to explain the rise of the movement, they have become a little vague. Lyndsay Farrall and Donald MacKenzie,²⁸ for example, have described the eugenists as engaging in 'middle class radicalism' and in 'middle class ideology' respectively. Middle class radicalism is said to have reigned by Farrall because the eugenists had nothing to gain personally from their

advocacy of eugenics, whereas Mackenzie sees the eugenisists as advancing their own interests in two ways: (i), directly, by arguing for, say, reduced taxation upon the middle classes, and (ii), indirectly, by demonstrating to the ruling or capitalist class that they (the professional middle classes, and, in particular, the fractions thereof whose claims to status relied on scientific knowledge) were the people equipped to deal with the social threat posed to the capitalist order by the existence of an undisciplined and frequently unemployed fraction of the working class that was supposedly growing ⁱⁿ the great cities of Britain. Its existence and extent, of course, had been recently uncovered by statisticians like Booth and Rowntree.

Farrall does not offer a strong explanation of the post-war failure of the eugenics movement. Mackenzie argues that, after the first war, the threat to the capitalist order began to come from a very different direction - namely from an organised labour movement - and, accordingly, that the traditional programme of negative eugenics backed by the eugenisists no longer offered capital a solution for its problems. Thereby, it is said, the movement lost in influence and began to become an increasingly academic movement. Searle explains the demise of the eugenisists' influence by referring to a range of factors, including increasing academic specialization in the post-war period (which rent the synthesis that eugenics had once represented) and the realization by scientists that 'the mechanism of inheritance among human beings was more complex than had once been claimed' - a factor which cooled the enthusiasms of would-be eugenisists.

There is no denying that a central feature of the eugenics movement was ^{the presence} of middle class professionals offering biological solutions to apparent flaws within the composition and behaviour of the would-be 'imperial race'. We are, after all, dealing with the England ²⁹ of Scouting for boys - with all its imperial enthusiasms - and of Kipling. But, once we get beyond this, we run into the vaguenesses incorporated into Mackenzie's and Farrall's explanations. Farrall is doubtless right when he suggests that eugenic propagandising brought no direct benefit to the propagandiser. In this sense then, the eugenisists might be described as indulging in 'middle class radicalism' - but this does not explain why they did it. Mackenzie seems to offer an explanation by suggesting that being a eugenisist brought a profit in the shape of increased status for one's social class (in this case,

middle and professional). In his view then, eugenists were short-term altruists, but long-term self seekers. While not gaining personally, they presented social problems and solutions thereto in a eugenic manner as their so doing gained favour for their class from a ruling capitalist class. When eugenic nostrums became inappropriate ideological weapons for the capitalists, the steam went out of the eugenics movement which fell in esteem among the professional middle class.

It is hard to know quite what to make of the explanations given of the rise and fall of the eugenics movement. It certainly does seem to be the case that, in the period prior to the first war, there were fears afoot that the 'imperial race' was slipping. Lord Rosebury,³⁰ no less, spoke of 'a perpetual lowering of the vitality of the imperial race', and there were wide-spread desires to get the 'rotten apples' from the social barrel. On the other hand, it seems unlikely that a small social 'residuum' was perceived by the capitalists as a leading threat, when the years 1911-12, for example, saw strikes 'among seamen, dock and transport workers, and a general railway strike'.³¹ Similarly, it seems likely that fears about the decline of the imperial race must simply have been arrested by the war, which itself, was hardly eugenic in its consequences. After all, it was the great race-rival, the Germans, that had been overthrown. Added to this, as factors that should be explored, there are surely the changed values, the destruction of old certainties that eventuated from the war. We are into the period of the Waste Land. A possibly typical example of these cultural changes is F.A.E. Crew,³² the noted geneticist, whose article of 1918 is perhaps the most civilised and eloquent piece ever to appear among the dreary pages of the Eugenics review: its style and spirit remind one of George Orwell. After denouncing Pearson, Galton, Bateson and Spencer as being on all fours with Haeckel and Treitschke, he argued against simplistic and biological interpretations of society which so many eugenists had peddled

We believe that equality of opportunity has discovered unsuspected capacity, and that the average man can acquire the average mental attainments of the classes, up to this, termed the intellectual. Of these matters, youth may know more than its masters, for it was youth that went through the fire in Flanders.

As to Searle's suggestion³³ that increasing complexity in genetics was a major factor in the fall of eugenics, it remains for evidence to be produced. As we shall see, however, some leading geneticists were able to survive the war with eugenic fervour undiminished.

One point does need to be stressed. I have mentioned at an earlier point that Weismann's views on heredity - which denied the possibility of the inheritance of acquired characters - became prominent in Britain in the nineties. Weismann and Spencer, for example, fought many rounds over this issue in the periodical literature of the nineties.³⁴ But, there seems little chance of ascribing the growth of the eugenics movement to the growing acceptance of Weismann's views. In the first place, there is a point of logic, namely that acceptance of the theory of the continuity of the germ-plasm does not per se commit one to ascribing, say, variance in IQ to genetical rather than environmental factors. But, more importantly, we find that many leading eugenists did not believe in the inviolability of the germ-plasm. Take, for example, the ideas of A.F. Tredgold the leading neurologist, who was a key witness before the Royal Commission on the care and control of the feeble-minded.³⁵

the causes of germinal variation, whether retrogressive or progressive, are very imperfectly known, and there is urgent need of their study... My own observations have led me to the conclusion that they are to be found in the environment, using this term in its widest sense, and that the psychopathic diathesis which reaches its culmination in amentia is, at the beginning, dependent upon disease or disorder of the metabolism induced by external causes and faulty modes of life.

Tredgold, interestingly though not untypically, demanded short-term segregation to stop the propagation of the mentally defective, but, concurrently, he also stressed the importance of making those environmental reforms that would stop the further production of defective germ-plasm.

Eugenics as a would-be science.

I have noted that Pearson, once he had assumed control of the Eugenics Laboratory, set to work to establish eugenics as a science, as the key social science, based on the rock-hard foundations of statistical method. Eugenics, it might be said, was seen as having normative features, for it could act as a guide to policy. Karl Pearson was quite firm on this point when enlightening undergraduates.³⁶

Of one thing, however, I feel sure, that no judgement will lead to lasting social gain which is reached by appeal to the emotions, which is based on inadequate knowledge of facts, or which collects data with the view of supporting any preconceived opinion. In short, on all these grounds we see that what is needed is the academic judgement. You cannot settle such essential problems of society as alcoholism, tuberculosis, mental defectiveness, or the changing status of women, by oratory in the market-place. I claim that these things must be studied in University laboratories, where Oxford shall check the results of Cambridge, and London correct both of them, if need be.

Pearson, evidently, was expecting a proliferation of eugenics laboratories. Let us now see what the academic discipline of eugenics comprised in London, and whether it eschewed emotions and preconceived opinions.

The works of the Galton Laboratory were contained in four sets of publications. There was a 'memoir series', a 'lecture series', a series entitled 'questions of the day and fray' and the ominous 'studies in national deterioration'. After 1920, of course, there was also the Annals of eugenics, but this takes us beyond the general limits of this study.

In general, the science of eugenics sought to show that a wide range of human conditions were due predominantly to nature rather than to nurture. This was a preconceived opinion. More specifically, the eugenists tried (i) to show that Britain was in danger from a growing super-fertility of the least desirable elements of society, (ii), to demonstrate the high heritability of mental characters such as intelligence, (iii), to show that various medical and social problems, such as tuberculosis, alcoholism, criminality and insanity were due to bad heredity rather than to bad environment - and that they should be dealt with accordingly, and (iv), that Mendelian analyses of human heredity were often wrong.

In what follows, I shall seek to indicate some of the general lines which the endeavour of eugenics took when addressing these issues. I shall look firstly at items (i) and (ii) above, and secondly at item (iii), and will suggest that the availability of sophisticated statistical methods was frequently over-ridden by simple failures in logic.

(a) Intelligence, nurture and social decay.

The first main thrust in the establishment of scientific eugenics came in Pearson's 1903 Huxley lecture³⁷ on the 'Inheritance of the mental and moral characters in man, and its comparison with the inheritance of physical

characters.' The argument, employed to show that intelligence was bred, not taught, was as simple as it was bad, and may be schematised as follows.

Premiss (1)

Several physical characters, height for example, have a normal distribution. It may be assumed that eye-colour depends on some normally distributed quantity.³⁸

Premiss (2)

The correlation between brethren for such characters is approximately 0.5. [Exact values given below.]

TABLE III.
Inheritance of the Physical Characters.
School Observations on Children.

Character.	Correlation.		
	Brothers.	Sisters.	Brother and Sister.
Health	.52	.51	.57
Eye Colour	.54	.52	.53
Hair	.62	.57	.55
Hair Curliness	.50	.52	.52
Cephalic Index	.49	.54	.43
Head Length	.50	.43	.46
Head Breadth	.59	.62	.54
Head Height	.55	.52	.49
Mean	.51	.53	.51
Athletic Power	.72	.75	.49

Premiss (3)

It is reasonable to assume that intelligence, as measured by teachers' assessments is also normally distributed - as are other psychical characters.

Premiss (4)

The correlation between brethren for such characters is again about 0.5. (see table below).

TABLE IV.

Inheritance of the Mental Characteristics.

School Observations on Children.

Image removed due to third party copyright

Premiss (5)

The value for eye-colour is unaffected by environmental factors. It is, says Pearson, implausible to suppose otherwise.

1st Conclusion³⁹

(Because of (15) and (2)) 'I am compelled to conclude that the environmental influence on physical characters, however great in some cases, is not to a first approximation a great disturbing character when we consider coefficients of fraternal correlation in man'.

2nd Conclusion 40

The near equality of correlations for mental and moral characters means that, 'we are forced absolutely to the conclusion that the degree of resemblance of the physical and mental characters in children is one and the same.'

Certainly, the argument, as developed, is not entirely clear, but has the following thrust. A basic idea is that the observed correlations between relatives are controlled by the relative forces of heredity and environment. In the case of eye-colour, immunity from environmental pressures is guaranteed by the nature of the attribute in question. Hence the correlation in this case gives a measure of the degree of resemblance produced by heredity acting alone. Another basic idea is that of coincidence. Eye colour and other characters have the same correlation. To assume that the correlations in other characters were due to balances of heredity and environmental factors yielding always correlations of about 0.5 is outrageous and ad hoc, so it makes sense to suppose that the uniformity of correlations is due to like causal forces operating - in this case, the forces of heredity alone. We shall return to these points, but, for the moment, consider the extended conclusion drawn from this. 41

If the conclusion we have reached to-night is substantially a true one, and for my part I cannot for a moment doubt that it is so, then what is its lesson for us as a community? Why simply that geniality and probity and ability may be fostered indeed by home environment and by provision of good schools and well equipped institutions for research, but that their origin, like health and muscle, is deeper down than these things. They are bred and not created. That good stock breeds good stock is a commonplace of every farmer; that the strong man and woman have healthy children is widely recognised too. But we have left the moral and mental faculties as qualities for which we can provide amply by home environment and sound education.

It is the stock itself which makes its home environment, the education is of small service, unless it be applied to an intelligent race of men.

Our traders declare that we are no match for Germans and Americans. Our men of science run about two continents and proclaim the glory of foreign universities and the crying need for technical instruction. Our politicians catch the general apprehension and rush to heroic remedies. Looking round impassionately from the calm atmosphere of anthropology, I fear there really does exist a lack of leaders of the highest intelligence, in science, in the arts, in trade, even in politics. I do seem to see a want of intelligence in the British merchant, in the British professional man and in the British workman. But I do not think the remedy lies solely in adopting foreign methods

of instruction or in the spread of technical education. I believe we have a paucity, just now, of the better intelligences to guide us, and of the moderate intelligences to be successfully guided. The only account we can give of this on the basis of the result we have reached to-night is that we are ceasing as a nation to breed intelligence as we did fifty to a hundred years ago. The mentally better stock in the nation is not reproducing itself at the same rate as it did of old: the less able, and the less energetic, are more fertile than the better stocks. No scheme of wider or more thorough education will bring up in the scale of intelligence hereditary weakness to the level of hereditary strength. The only remedy, if one be possible at all, is to alter the relative fertility of the good and the bad stocks in the community. Let us have a census of the effective size of families among the intellectual classes now and a comparison with the effective size of families in the like classes in the first half of last century. You will, I feel certain, find, as in the case of recent like censuses in America, that the intellectual classes are now scarcely reproducing their own numbers, and are very far from keeping pace with the total growth of the nation. Compare in another such census the fertility of the more intelligent working man with that of the uneducated hand labourer. You will, I again feel certain, find that grave changes have taken place in relative fertility during the last forty years. We stand, I venture to think, at the commencement of an epoch, which will be marked by a great dearth of ability. If the views I have put before you to-night be even approximately correct, the remedy lies beyond the reach of revised educational systems; we have failed to realize that the psychical characters, which are, in the modern struggle of nations, the backbone of a state, are not manufactured by home and school and college; they are bred in the bone; and for the last forty years the intellectual classes of the nation, enervated by wealth or by love of pleasure, or following an erroneous standard of life, have ceased to give us in due proportion the men we want to carry on the ever-growing work of our empire, to battle in the fore-rank of the ever intensified struggle of nations.

Do not let me close with too gloomy a note. I do not merely state our lack. I have striven by a study of the inheritance of the mental and moral characters in man to see how it arises, and to know the real source of an evil is half-way to finding a remedy. That remedy lies first in getting the intellectual section of our nation to realize that intelligence can be aided and be trained, but no training or education can create it. You must breed it, that is the broad result for statecraft which flows from the equality in inheritance of the psychical and the physical characters in man.

It seems to me that there are at least two confusions here.

(1) What is presumably significant about the eye-colour correlation is that it is invariant. The correlation coefficient linking parent and offspring in respect of eye-colour, it seems reasonable to suppose, does not change with environmental change. But, Pearson's data give him no grounds to attribute

a similar invariance to the coefficients for intelligence and other psychic characters. Yet, without this result, his data *lacks* eugenic significance. (2) Even if such an invariance were established, it would not in the least follow, as Pearson appears to have thought, that the best way to boost national intelligence was by breeding. Correlation coefficients connect deviations from the mean, but say nothing about the mean. Logically, it is quite possible that father-son correlations in respect of height are the same now in Britain as 300 years ago. In the meantime, average stature has increased, but due to better conditions, not to selective breeding from the tallest Elizabethans.⁴²

A third difficulty may also be mentioned. This is that the correlations in question were derived using Pearson's tetrachoric method, embodying the dubious assumption of premiss (1) that eye-colour has an underlying normally distributed basis. Yule pointed out in his papers that Pearson's data gave little or no reason to suppose this assumption true.⁴³

Here then, we see something of the loose way in which the academic eugenis~~t~~s were prepared to put their arguments. Above all, we see the message they wished to impart - national decline was due to too much breeding by the stupid and too little breeding by the intelligent. The high 'heritability' of intelligence showed how serious a complaint this could be. Pearson's views, it seems, cut *little ice* with the Interdepartmental Committee appointed in 1905 to inquire into the physical deterioration of the British people. They announced that⁴⁴

'the Committee have not been able to obtain decided confirmation of this now.'

It was possibly in order to rebut this view that Pearson's lieutenant David Heron set to work to produce the first of the 'Studies in national deterioration', namely the influential memoir 'On the relation of fertility in man to social status and on the changes in this relation that have taken place in the last 50 years', which appeared in 1906. This addressed the issue of whether or not the most fertile component of the population was 'the better or worse portion' of the community.

But, who were the 'better or worse' portions of the community? What were the indices of standing in this respect? Heron, of course, had to operate within the bounds laid down by data already collected - in this case, those contained within (i) the Report of the Registrar General, 1901: (ii), the

Census of England and Wales, 1901. (iii) the Report of the Medical Officer of Health of the County of London for 1901, (iv) the Report of the Registrar General for 1851, and, (v) the census of England and Wales for 1851.

Within the categories offered by these stern authors, Heron took as his indices of quality⁴⁶ the following statistics, gathered for every London borough.

(A) 'Of health and education'

- (1) The number of professional men per 1000 occupied males.
- (2) The number of female domestic servants per 100 females aged 15 and upwards.
- (3) The number of female domestic servants per 100 families.

(B) 'Of poverty and lack of culture'

- (1) The number of general labourers per 1000 occupied males.
- (2) The number of pawnbrokers per 1000 occupied males.

(C) 'Of thriftlessness and poverty'

- (1) The number per 100 in each district living more than 2 in a room.
- (2) The number of deaths in each district of children under 1 year per 1000 births.

With these statistics, gathered for every London borough at the two periods, Heron could begin the automatic and systematic application of the method of correlation coefficients learned from Pearson. An example of his data is given below, for the year 1901. It will be seen that, of all the London boroughs, Hampstead - then as now, favoured by the University College professoriate - was clearly the most superior as judged by Heron's criteria; a veritable paradise, a land running with professional men and female domestic servants.

The correlations obtained are unsurprising - except in one respect - that of cancer.

Heron considered the correlation between the birth rate per 100 married women between 15 and 54 in each district and the following factors, measured over all the boroughs. The results were as follows:⁴⁷

<u>Factor</u>	<u>Correlation</u>
A(1) See above	-0.781 \pm 0.051
A(2) " "	-0.764 \pm 0.054
B(1) " "	-0.517 \pm 0.095
B(2) " "	0.660 \pm 0.073
C(1)	0.697
C(2)	0.500 \pm 0.097

Image removed due to third party copyright

Typical statistics from Heron's 1906 Study in national deterioration.

The conclusion, Heron said, was apparent

In those districts where the professional classes are most numerous and where many domestic servants are kept, there the married women have fewest children. The relation is an exceedingly close one and is obviously most significant.

Where labour is of the lowest type, where poverty leads to the pawnbroker and forces the child at the earliest possible age into employment, there the married women have most offspring.

The worse the sanitary conditions under which the people live, and the worse their physical and mental health, the higher is the birth rate.

In all, said Heron, his statistics demonstrated that in every case, socially and physically undesirable features were correlated with the intensity of the birth rate. Most importantly,

The wives in the districts of least prosperity and culture have the largest families, and the morally and socially lowest classes in the community are those which are reproducing themselves with the greatest rapidity.

This state of affairs, Heron could not but deplore: and what made it worse was that the relationships between physical and social degeneracy on the one hand, and fecundity on the other appeared to be deteriorating. Heron sought to demonstrate this. A preliminary move was to 'purify' his statistic by taking account of the different age-structures among the wives living in the different boroughs. This, in a sense, was not an essential move, as all that really mattered from the eugenic standpoint was the general productivity of the different components of society, and not whether differentials were due to earlier marriage among the lower classes alone, or to earlier marriages and a generally higher rate of fertility. This new statistic could be obtained by the method of partial correlation, allowing Heron to calculate the correlation between birth rate and the various social factors already alluded to in the different boroughs for women of constant age. This could be done for the two periods - 1901 and 1851, with the following results. (The gross, total, correlations are given in the first table, the partial coefficients in the second).

The first table indicated a tendency for correlations to double over the period. And, as Heron noted,

TABLE IX. Comparison of Correlations of Birth-rate and Social Status in
1851 and 1901.

Image removed due to third party copyright

TABLE X. Effect of mean age of wives on coefficients of correlation in
1851 and 1901.

Image removed due to third party copyright

Tables taken from Heron's 1906 Study in national deterioration.

The causes which lead the poorer stocks of the community to reproduce at a greater rate than the better stocks have increased in effect during the last fifty years by nearly 100 percent.

The partial coefficients (though calculated on a partly assumed basis) seemed equally frightening to Heron. For, as he put it.⁴⁸

Every one of the 1851 correlations is now reversed in sign. In other words, if the influence of social status on age of wives had been of the same intensity as in 1901 it would have more than accounted for the observed correlations between low social status and high birth rate.

What Heron meant by all of this was that the sources of correlation seemed to have changed over the period. In 1851, the taking of wives of constant age gave correlations between fertility and social position (in the London boroughs as a whole) that were in a 'favourable' direction. The generally 'unfavourable' nature of the correlation coefficients was due to the different ages of marriage of women in the different boroughs. The poor married early, and, so, could keep at the business of procreation for longer than the professional classes, but not, it seemed, at an inherently higher rate. By 1901, however, the partialling out of age as a factor indicated that there still remained significant correlations between social conditions and fertility - and that these were in the 'wrong' direction. As Heron said,

we have to deal with a reduced fertility in the more cultured, the more prosperous, healthy and thrifty classes of the community which cannot be accounted for by the variation in the mean age of the possibly reproductive wives.

The final section of Heron's report was a typically eugenic document. It first of all stressed the need for the establishment of further bureaucracy to collect the statistics which would enable the eugenicists to pursue further their vital investigations. It stressed that there was a much higher birth rate amongst 'undesirable' as opposed to desirable elements, saying that,

The relationship between inferior status and high birth rate has practically doubled during the last fifty years, and it is clear that in London at least the reduction in size of families has begun at the wrong end of the social scale and is increasing in the wrong way. I have brought forward evidence to show that the birth of the abler and more capable stocks is decreasing relatively to the mentally and physically feeble stocks.

Physical and mental characters, tendencies to health and disease, intellectual and manual capacities are undoubtedly inherited.

A higher net fertility is shown, under at any rate the present social conditions of a large city, to be very markedly correlated with most undesirable social factors.

These points, he claimed,⁴⁹ 'indicate sources of national deterioration which the statesman and social reformer must be prepared to consider, and consider quickly.'

The central weakness in Heron's work, of course, was that it assumed a correlation between, say, natural innate, genetically determined intelligence and social position. It assumed that the professional man's skill was possible only ^{or mainly} because the professional man was genetically superior. It assumed that the poor man's fertility was serious because poverty was a measure of innate, genetically determined ability. Karl Pearson, under an extremely favourable light might be supposed to have offered some evidence that ability was hereditarily determined, but no one had yet shown any tie-up between lower-classedness and poverty and innate lack of intelligence. It was an assumption, not anything that had been demonstrated, and, as far as I can see, was an assumption whose dubious status was never repaired by the academic eugenisists. Their main attempt was perhaps the Eugenics Laboratory memoir number 18 of 1913, 'On the correlation of fertility with social value. A cooperative study', produced jointly by Pearson, Ethel Elderton, Amy Barrington, Gertrude Jones, Edith La Motte and Harold Laski⁵⁰ (who, in his early days, was a keen eugenisist). This, however, suffered from the same limitations as the Heron study - for it assumed, ceteris paribus, that wage-rates varied as eugenic worth. Yet, this was a proposition which remained to be shown.

In conclusion, there are a few points that can usefully be made concerning the academic eugenisists' attacks on the issue of the heritability of intelligence and on the ~~question~~ of the relation between intelligence and fertility; one of these is a logical point, and the others are historical. In the first place, it seems clear that the standards of argument deployed were frequently low, the use of sophisticated mathematics notwithstanding. Secondly, it is interesting to note that the academic eugenisists perceived themselves as socialists and as pioneers in a new style of social science. Pearson could contrast himself with the Fabians, regretting the day when a section of the pre-Fabian Brotherhood of Common Life 'preferred the Fabian

policy of discussing to that of practicing socialism'.⁵¹ That the eugenists saw themselves as potential exercisers of power is made quite apparent in many tracts, which frequently contain sentiments similar to the following.⁵²

I claim - not for the old sociology with its philosophical and verbal disquisitions - but for the new medico-social data and the new calculus of correlation, a recognised place in science: a right to speak in the future with some authority in matters of social reform, and even on points of supreme national welfare. I believe that the day for acting merely on a consensus of opinion based on rhetorical or ~~for~~ emotional appeal of a political or philanthropic character is passing by....

Thirdly, it is worth noting that the eugenists' tradition of stressing the dangers of a differential fertility between social classes seems to have had quite an impact. One has only to look at the Report of the Royal Commission on population in 1948 to see that the matter of differential fertility was still seen as important, with evidence as to this importance taken from Godfrey Thomson, R.A. Fisher, Cyril Burt and others. A more immediate impact was made upon British psychologists, and will be discussed in the next chapter.⁵³

(b) The eugenic analysis of social problems.

The basic style of eugenic analysis has already been seen - namely that of calculating correlation coefficients and interpreting them. But, to acquire a better comprehension of this style of procedure, it seems sensible to examine the work of the eugenists outside of the specialised area of intelligence. Assuredly, there is a wealth of potential example. The eugenists worked on alcoholism incurring the wrath of Maynard Keynes and Marshall the economists,⁵⁴ on insanity, on eye sight and on many other topics. As an example, however, I would like to take the case of tuberculosis. This is a particularly enlightening example, as it shows the eugenists' commitment to dealing with the issues of the day, or, as they would say, the 'questions of the day and fray'. For, in 1911, the National Insurance Act made a great step, by providing for money to be set aside at the rate of 'one penny per insured person in the United Kingdom' for use in tuberculosis research.⁵⁵ Indeed, the Medical Research Council owes its origins to government considerations of the problems of tuberculosis in 1913. Tuberculosis therefore was in the air in more senses than one, and, moreover, the air must have been laden with the scent of

possible research grants. Perhaps unsurprisingly therefore, the academic eugenisists published a tract on 'Tuberculosis, heredity and environment in 1912'. This, as it transpired, was the first of several publications on tuberculosis, ^{and an} archetypal one. In the paper, Pearson argued that published statistics for the cure rates produced by the then expanding sanatorium treatment of tuberculosis gave⁵⁶

no evidence that this treatment is producing marked results; they supply no refutation of the position that the fall in pthisis [Pearson's term for pulmonary tuberculosis] is due not to the reduction of infection but to the development by heredity of a racial immunity.

Pearson, it seems clear, was offering a Darwinian explanation of the steady fall in the death rate from tuberculosis which, by 1912, had been continuing for many years. But, this was not the main thrust of his tract, intriguing though it be. The bulk of the tract was dedicated to showing that there existed in the population hereditable levels of susceptibility to tuberculosis - in other words, that an individual's developing the disease or not depended largely upon his hereditary susceptibility to succumb to the supposedly omni-present bacillus. This was done by offering statistics which showed that husbands were far less likely to have infected wives than infected offspring. This, as usual, was shown via the method of tetrachoric correlation, and the result was interpreted to mean that heredity was a greater determinant of the tuberculous condition than was environment.

The practical conclusion followed inexorably:⁵⁷

Everything which tends to check the multiplication of the unfit, to emphasize the fertility of the physically and mentally healthy, will pro tanto aid Nature's method of reducing the phthisical death rate. That is what the eugenist proclaims as 'the better thing to do', and £1,500,000 spent in encouraging healthy parentage would do more than the establishment of a sanatorium in every township.

Now, the whole subject of the decline of tuberculosis remains a fascinating one, though modern writers⁵⁸ seem to accept both that susceptibility is hereditary and that the decline has been due to environmental improvements rather than Darwinian processes. But, what is significant here is that the eugenists made again one of the loose inferences which we have seen already in a different context - namely

heritable to the
conclusion that
selective breeding
is

that one may infer from the circumstance that condition x is the best way to enhance or to reduce the level of x in the population. This might be called the 'eugenic fallacy', and we find that it was repeated time and again.⁵⁹ Eugenic fervour consistently bettered (or, perhaps, battered) logic.

At this stage I propose to say no more about the academic side of eugenics, except, of course, to make the usual incantations about the need for 'further research' into its development and influence. Just what this influence has been is hard to say, but it is easy to suppose that, for example, recent investigations into the possibility of hereditary criminality lie in a direct line of descent from the work done by Charles Goring, on behalf of the Home Office, in Pearson's laboratory. This work, published as The English convict: A statistical study discredited Lombroso's view that there was a distinctive criminal physiogony, and advanced with evidence⁶⁰ the view that criminality was restricted to 'particular stocks or sections of the community'.

Eugenics as a midwife of science.

There is little profit in discussing whether or not academic eugenics ever constituted a science. If it did, then it was defective in parts. More profitable perhaps is the investigation of the effect of the eugenics movement on other branches of 'genuine' science. This was considerable, though not overwhelming. It deserves attention as it is a prima facie example of 'external' ideological considerations shaping the course of scientific development, a state of affairs held by some to be of interest.

A first point concerns the scientific membership of the Eugenics Education Society - for it was certainly high. An indication of this is given⁶¹ by the membership of the 'General Committee' which helped organise the 1912 International Congress; this included Francis Darwin, Havelock Ellis, Sir James Barr, R.C. Punnett, Arthur Schuster, A.F. Tredgold and W.C.D. Whetham. A full list of 'scientific' eugenis~~t~~s would be an extremely lengthy one. Interestingly, it was not simply metropolitan scientists who joined. The sixth annual report⁶² of the E.E.S. shows that the Birmingham branch had as a vice-president Sir Oliver Lodge, and we know that the Liverpool branch numbered Cyril Burt the psychologist and the future

Sir Charles Sherrington, Britain's most distinguished neurophysiologist. An active London member was A.M. Carr-Saunders, future director of the London School of Economics.

From all of this, one might expect to find the eugenics movement having some impact upon the scientific work of some of its members, with, perhaps, biology, psychology and social sciences being, a priori, the best places to look. I have decided to leave social science on one side,⁶³ though there are many interesting and correct remarks on the relationship between eugenisists and sociologists in Professor Abrams' excellent book⁶⁴ on the Origins of British Sociology.

I will address the relations between eugenics and psychology in the next chapter, which is dedicated to tracing the fate of biometric methods when passed onto the discipline of psychology. In the final part of this chapter, I shall examine the relationship between eugenics and genetics, focussing for reasons that will become obvious, on the work of R.A. Fisher.

The relations between the eugenics movement and the British pioneers of Mendelism seem, on the whole, to have been characterised by a guarded friendship. Works on genetics by those pioneers - like, for example R.H. Lock - frequently contain a mention,⁶⁵ or even a chapter, that discusses the possible benefits to be derived from the rational application of the new genetic knowledge. On the other hand, these same works also tend to stress caution, and a typical note was struck by R.C. Punnett in 1912, when, at the International Eugenics Conference,⁶⁶ he warned the assembled delegates that

Except in very few cases, our knowledge of heredity
in man is at present far too slight and too uncertain
to base legislation upon.

But, if a benign scepticism was the rule, it is worth noting that Bateson, the doyen of British Mendelism, was extremely hostile to the eugenic enterprise. This was not because Bateson doubted that men differed in abilities on an hereditary basis, nor because he was an egalitarian, it was because he had a fine, aristocratic disdain for the eugenisists themselves. Bateson was an ardent anti-democract, capable of writing that⁶⁷

The aim of social reform must be not to abolish class,
but to provide that each individual shall so far as
possible get into the right class and stay there, and
usually his children after him.

Progress, in his view, was due to unusual men

It is upon mutational novelties, definite favourable variations, that all progress in civilisation and in due control of natural forces must depend. How will they fare in a socialistic community?

These men, he felt, were not ones that recommended themselves to the eugenisists. Bateson was a talented artist and a recognised connoisseur who became a trustee of the British Museum, and he allied himself with the bohemian artist. The eugenisists, he felt, were bent upon eliminating such bohemians, they were men who shared the pauper spirit of the American prohibitionists. Again,⁶⁸

though each of us has his personal predictions we can only make rough estimates of the worth of the several types and of their value to the world. Quantitative reckonings are still very far off, and meanwhile we must remain content with academic aspirations, praying only that in that day humanity may not be measured by the scale which would be appropriate to a Charity Organisation Society or a Board of Guardians, who I am told are able to distinguish the deserving from the undeserving poor.

R.A Fisher was of a very different stamp.⁶⁹ For a start, he was not a biologist by training. He was the youngest of eight children and the son of a partner in a distinguished firm of auctioneers and of a Solicitor's daughter. From an early state he showed mathematical precociousness and moved on from Harrow School to Gonville and Caius College Cambridge, graduating with distinction in 1912 in the mathematics tripos. He was a non-combatant in the first war, as he suffered from very poor eyesight, and spent the duration as a mathematics master in a number of English public schools. He married in 1917, and, in 1919, took up the post of statistician at Rothamsted Experimental Station created then by its director, Sir John Russell. Here he stayed until 1933, developing his distinctive school of statistical thought, and publishing several highly influential works, such as his Statistical methods for research workers of 1925. He was elected a fellow of the Royal Society in 1929, and, in the following year, published his famous Genetical theory of natural selection which is often depicted as having repaired the rift between Darwinian selection and Mendelian theory which appeared in the works of men like the evolutionary discontinuist Bateson. In 1933, he assumed

the Galton chair at University College, and held this until 1943, when he took over the Arthur Balfour chair of genetics at Cambridge. On retirement from this, he went to work at the University of Adelaide, dying, in Australia, in 1962. In short, he had a long and extraordinarily distinguished career that led him to a knighthood. Like Pearson he made massive contributions to statistics, and, like Pearson, allied this statistical expertise to the study of biological phenomena. Unlike Pearson, he appears to have been a political conservative, and, whereas Pearson was fond of denouncing religion, Fisher seems to have approved of it, being known for detailed scriptural knowledge and for regular attendance at chapel. Like Pearson, he was prone to write with harshness and acerbity about those with whose views he was out of sympathy. Pearson was sometimes on the receiving end.

It has for long been known that Fisher's entrance into statistics came about through his reading Pearson's papers while he was still an undergraduate - and he published his first paper 'On an absolute criterion for fitting frequency curves' in 1912. This was just the first of a long series of statistical papers that were to gain him at first the friendship, and, later on, the enmity of Pearson. In this work, however, I will not attempt to discuss Fisher's statistical work, particularly as it has been the subject of historical and logical analysis by many distinguished writers.

What I do want to concentrate upon is Fisher's early work in genetics, and it will be remembered that his first contribution in the field⁷⁰ was his paper of 1918 on 'The correlations between relatives on the supposition of Mendelian inheritance'. This it will be recalled, completed that Mendelian analysis of normally distributed continuous variation which had been pioneered by Yule, and which had been heavily criticised by Pearson. Generally, Fisher is seen as the man who showed conclusively that Mendelism could cope with the known facts of continuous variation, and his 1918 paper is usually seen as a work of genius - which it undoubtedly is.

Naturally, we have now to ask the same question about Fisher that was posed about Pearson in chapter four. Why should Fisher have become involved in biology, in genetics, when he was a mathematics graduate? As I have stressed, at the period, there were no career openings for mathematicians

With biological concerns, and the combination of Mendelism and mathematics might be thought especially ~~in~~propitious, given the anti-Mendelian stance of Pearson, the powerful leader of the biometric school. It seems highly improbable, therefore, that Fisher was trying to advance a career by turning to Mendelism.

Some writers have noted a boyhood interest in biological matters, but, until Fisher's papers become available, it is hard to say much about this that is of any moment, and it seems eminently more sensible to attempt to connect Fisher's genetical labours with other factors that can be better documented, notably with his life-long commitment to eugenics, which is perhaps most conveniently observed in the ^{not}five chapters of his Genetical theory of natural selection,⁷¹ where eugenics and genetics are mobilised to account for the very rise and fall of great empires. As early on as 1926, Fisher had published no fewer than fifteen papers in the Eugenics review and was becoming a leading member of the Eugenics Education Society.⁷²

Fisher's first moves in eugenics came while he was still an undergraduate at Cambridge, when, in 1911, a group of undergraduates approached Professor Inge, Professor Punnett and W.C.D. Whetham with a view to forming a University Eugenics Society. The Society was formed, and, with the assistance of men as eminent as John Maynard Keynes and Lord Rayleigh, began to hold meetings. One of the earlier talks was due to Fisher, who, in 1911, gave a paper on 'Heredity', in which he compared and contrasted biometric and Mendelian methods and looked forward to a synthesis of the two schools in the interests of eugenics. In his talk,⁷³ Fisher showed himself familiar with the work of Bateson and Pearson, and quite capable of dealing with technical issues in Mendelism such as intermediate dominance. In particular, he demonstrated an acquaintance with Pearson's 1903 Huxley lecture. Something of the depths of his commitment to eugenics may be gauged from another address, of 1912,⁷⁴ in which he urged upon his audience that they were the 'agents of a new phase of evolution', whose mission it was to spread abroad 'not by precept only, but by example, the doctrine of a new natural ability of worth and blood'. In these phrases, Fisher did little more than to prefigure later eugenic utterances, which show him as advocating income-related family allowances, the restriction of professional opportunities to the offspring of professionals, and the view that eugenic strategies alone could prevent the fall of great civilisations.⁷⁵

It seems therefore, that there is every reason to suppose that Fisher's entry into ~~genetics~~ was a consequence of his conversion to the eugenic philosophy. After all, the problem tackled in his great paper of 1918 was outlined in 1911 in the Cambridge Eugenics Society, and, moreover, the paper was first presented to the Royal Society of London by the eugenicist Whetham. When the paper was withdrawn, after adverse criticism from Punnett and Pearson,⁷⁶ who acted as Royal Society referees, it was another leading eugenicist - Major Leonard Darwin - who advised Fisher to persevere and have his work published under the auspices of the Royal Society of Edinburgh.

Now, if all that could be said about Fisher's paper in this respect was that it arose out of eugenic concerns, we would, I think, still have a significant result - a good case of internal developments being due to 'external' factors. But, I consider, and I would like to suggest, it seems possible to make a stronger connection than this between Fisher's ideology and his work as represented in the 1918 paper.

To appreciate this, it is first of all necessary to understand the key problem facing serious eugenicists in the early 20th century. They were in the position of wishing to claim that the only effective forms of social policy were based in selective breeding. This, by turn, was the consequent of a faith, that in human affairs, nature dominated over nurture. Clearly, they needed to establish a truth of this central dogma. Now, we know Fisher to have been a keen student of the attempts made to establish the point, and that, after Galton, the man who did the most in this direction was Karl Pearson.

Fisher, as noted, was aware of Pearson's work. But, judging from a paper⁷⁷ delivered to the Eugenics Education Society at about the same time as the publication of the 1918 paper, he was highly critical of Pearson's strategy. This, indeed,⁷⁸ was a rather unsophisticated one, which frequently took the hitherto unmentioned form of the presentation of two columns of observed correlations - one said to give the 'Strength of nature', and another said to give the 'Strength of nurture'. The first gave correlations like that between filial and paternal stature, the other gave correlations like those observed, in restricted areas of data, between 'keenness of vision and home environment' or between 'moral state of apprents and refraction of offspring'. The average value for the 'nature' correlations was about 0.5,

whereas the average value for the 'nurture' correlations was about 0.05. The conclusion to be drawn, Pearson suggested, was that it was 'quite safe to say that the influence of the environment is not one fifth that of heredity and quite possibly not one tenth of it'. And, moreover, what we might see as typically Fisherian consequences were seen to flow from this.

Hard environment may be the salvation of a race, easy environment its destruction. If you will think this point out in detail, I believe you will see the explanation of many great historical movements. Barbarism has too often triumphed over civilisation, because a hard environment has maintained, an easy environment suspended, the force of natural selection - the power of the nature factor.

Now, though Fisher endorsed Pearson's conclusion, he was dissatisfied with the argumentation that led to it, and, I wish to suggest, there is a very strong connection between his dissatisfaction with the argument and the very shape taken by his 1918 paper.

Here it becomes necessary to mention the nature of the 1918 paper. Its novelty did not lie in its having shown the consistency of Mendelism and observed biometric results. Rather, it lay in Fisher's analysis of phenotypic variance into a number of fractions - into environmentally caused variance, into variance due to additive genetic effects and into variance due to dominance effects.⁸⁰ His analysis offered the prospect of carrying out resolutions of observed variance into the different fractions. This was because he was able to construct a series of equations expressing these fractions as functions of correlation values. Thus, for example, he showed that when there was no assortative mating, the difference between the fraternal and the paternal correlation offered a quick way of estimating the various fractions. When assortative mating was allowed for, things became more complex - but the same general pattern held.

Application of the new calculus to Pearson's data suggested that the variance within the data for stature was due almost entirely to genetic effects. As Fisher put it,⁸¹

Image removed due to third party copyright

An examination of the best available figures for human measurements shows that there is little or no indication of non-genetic causes. The closest scrutiny is invited on this point, not only on account of the practical importance of the predominant influence of natural inheritance, but because the significance of the fraternal correlation in this connection has not previously been realised.

Clearly, Fisher felt that he had shown the eugenic point to be correct - nature dominated over nurture. And, from an accompanying paper, it becomes clear that he saw his new demonstration as filling the gaps in Pearson's work. For, it will be recalled, Pearson had operated by comparing the magnitudes of two sorts of correlation coefficients. The correlations measuring 'nature', however, had a value of only about 0.5. And, since the variance of the array of sons due to fathers with a given value was still $(1 - r^2)$ that of offspring in general, Pearson's proof seemed to leave approximately 75% of the observed variance unaccounted for. Thus, said Fisher, the road was left open to the anti-eugenicist to,⁸²

point to a dozen causes to which height or shortness is commonly ascribed, such as regular athletic exercise, or accidental illness in childhood, and it would be difficult to prove without a specially designed investigation for each alleged cause that these do not contribute the important proportions of the total. The task of ascertaining the importance of the environment in this way is an endless one, since new environmental causes could be suggested, each more difficult than the last to define, measure and investigate.

Here, it seems to me, we have it. Fisher, whose work was all along stimulated by his connections with leading eugenisists like Whetham and Darwin, saw his work as an enterprise which could do away with the need for this endless series of analyses of particular environmental features - for, had he not offered a method for evaluating the total effect of environment factors? His work was little short of an eugenic triumph. It was so, not because it showed that Mendelism and biometric results were compatible - which had been done before, even if not at such a level of sophistication. Rather, its merit lay in the way in which it showed that a Mendelian approach could resolve the longest-standing difficulty for the eugenist.

The consequences of this, I wish to suggest, is that we should desist from seeing Fisher's early work as primarily a contribution to 'pure' genetics, as a major staging post on the route to a neo-Darwinian synthesis. We should see it rather as a stunning contribution to eugenics. That is all I will suggest for the moment, though, were more space available, the tracing of intimate connections between the eugenic problem situation and the construction of theory would be fruitful. Another point worth following is Fisher's use of his method - for he was shortly to be involved with Hogben⁸³ in a prototypic version of the Jensen-Lewontin debate over heritability.⁸⁴ Here too, there is space for further discussion of the relation between science and ideology. For the moment, though, I want simply to suggest this significant restructuring of the historical perspective in which this landmark of genetics should be seen. Fisher was bringing Mendelism to the aid of eugenics. This, probably, was the most significant development sired on genetics by eugenics. It was this paper of Fisher's that was to provide the basis for modern discussions of 'heritability', so frequently mobilised in attempts to show that intelligence is due predominantly to genetic factors. In Britain, the greatest employer of Fisher's genetics was the psychologist Cyril Burt, also a fervent eugenist, whose work I shall discuss in the next chapter.

- 1 Leyden, 1976
- 2 Lyndsay Farrall, The origin and growth of the English eugenics movement, 1865 - 1925 (Ph.D. thesis, Indiana University, 1970) See also D. MacKenzie, 'Eugenics in Britain', Social studies of science 6 (1976), 499 - 532. MacKenzie's article cites a great deal of relevant secondary literature.
- 3 W.R. Greg, 'On the failure of natural selection in the case of man', Frazer's Magazine, 78 (1868), 353 - 362.
- 4 Alfred Marshall, cited in G. Steadman-Jones, Outcast London, Penguin books (1971), 123. See, generally, Jones chapter 6, 'The theory of urban degeneration'.
- 5 Llewellyn Smith, in Jones, op.cit. (footnote 4), 131.
- 6 Charles Darwin, The descent of man, 2nd. edn London (1906), 222. Generally, see Darwin's chapter 5 and its section on 'Natural selection as effecting civilised nations'.
- 7 A.R. Wallace, 'Human selection', Fortnightly review, 48 (1890), 325 - 327. Interestingly, Wallace regarded this paper as being 'though very short, the most important contribution I have made to sociology and the cause of human progress'. See A.R. Wallace, My life, London (1905), vol. 2, 209.
- 8 F. Galton, 'The possible improvement of the human breed under existing conditions of law and sentiment', reprinted in Galton's Essays in eugenics, London (1909).
- 9 For this, see Galton, op.cit. (note 8)
- 10 See, for example, Galton's paper on 'Local associations for promoting eugenics', in Galton, op.cit. (note 8), where he argues for the accumulation of 'considerable funds to start young couples of worthy qualities in their married life, and to assist them and their families at critical times'. (p. 108)
- 11 See A.S. Parkes, 'The activities of the Eugenics Society', Eugenics review, 60 (1968), 142 - 161.
- 12 For details of this event, see Problems in eugenics: papers communicated to the first international Eugenics Conference, Eugenics Education Society, London (1912), 2 vols.
- 13 See Searle, op.cit. (note 1), 11.
- 14 Taken from the Sixth annual report of the Eugenics Education Society.
- 15 For an analysis, see Farrall, op.cit. (note 2), also, Searle, op.cit. (note 1).
- 16 For details, see S.P. Davies, The mentally retarded in society, New York (1959). See chapter 6 for statistics.
- 17 For an account of the Royal Commission, see Kathleen Jones, A history of the mental health services, London (1972). See chapter 8 'Mental defectives'. Jones is rather vague about the events leading up to the establishment of the Commission, referring to 'growing public

- 19 Searle, *op.cit.* (note 1), 106.
- 20 Eds.S. and B.Webb, The break up of the poor law, being part one of the minority report of the poor law commissioners, London (1909), 300.
- 21 Searle. Churchill's plans are discussed on p. 108 and the whole of chapter 9 is given to a study of the 'Mental deficiency act, 1913'.
- 22 But, there is evidence to suggest that, by the thirties, it was once again growing in strength. Work is being done on this by Searle and others, but an introduction to the sorts of debates found in Britain on the topic of voluntary sterilisation of the unemployed is given in P.G.Werskey, 'British scientists and 'outsider' politics, 1931 - 1945', in ed. S.B.Barnes, Sociology of science, Penguin books (1972), 231 - 250.
- 23 For details of the hostilities between Pearson and the E.E.S., see Farrall, *op.cit.* (note 2). See also D.W.Forrest, Francis Galton. The life and work of a Victorian genius, London (1974). Chapter 18, 'The final years'.
- 24 The minute book is located in the offices of the Society, at 69, Ecclestone Square, London.
- 25 G.K.Chesterton, Eugenics and other evils, London (1922). In his Introduction, Chesterton insisted that eugenists can 'offer us nothing but the same stuffy science, the same bullying bureaucracy and the same terrorism by tenth rate professors that have led the German Empire to its recent conspicuous triumph'.
- 26 For details of the 'national efficiency' drives so prominent in Edwardian England, see G.R.Searle, The quest for national efficiency: A study in British politics and political thought, 1899 - 1914, Oxford (1971).
- 27 See the Report of the Inter-Departmental Committee on Physical deterioration, Parliamentary papers 1904 Cd.2175,xxxii.1. Also, 'Miles', 'Where to get men', Contemporary review, 81 (1902), 78 - 86. Also, B.B.Gilbert, The evolution of national insurance in Great Britain, London (1966), 84 - 86.
- 28 Farrall, *op.cit.* (note 2), MacKenzie, *op.cit.* (note 2).
- 29 See S.Hynes, The Edwardian turn of mind, London (1968), 26 - 27.
- 30 See op.cit. (note 27).
- 31 See, for example, L.C.B.Seaman, Post-Victorian England, London (1967), 40 - 42. And David Thomson, England in the nineteenth century, Penguin books (1975), 197 - 202.

- 32 F.A.E.Crew, 'A biologist in a new environment', Eugenics review, 11, (1919 - 1920), 119 - 123.
- 33 Searle, op.cit. (note 1), 115.
- 34 For references, see J.D.Y.Peel, Herbert Spencer: The evolution of a sociologist, London (1971). See chapter 6, 'Evolution', 131 - 165.
- 35 A.F.Tredgold, Mental deficiency (Amentia), 2nd. edn. London (1914) 462.
- 36 Karl Pearson, The academic aspect of the science of national eugenics (Eugenics Laboratory lecture series, 7) (1911), 19.
- 37 Karl Pearson, 'Inheritance of the mental and moral characters in man, and its comparison with the inheritance of physical characters', Jnl. Anthropol. Inst. of Gt. Britain and Ireland, 33 (1903), 179 - 237.
- 38 Pearson's practice was attacked by G.U.Yule in his 'On a property that holds good for all groupings of a normal distribution...', Proc. Roy. Soc., 77A (1906), 324 - 336. See also Yule's paper on the 'Influence of bias and personal equation in statistics of ill defined qualities', Jnl. Roy. Anthropol. Inst., 36 (1906), 325 - 381.
- 39 Pearson, op.cit. (note 37), 202.
- 40 Ibid, 203
- 41 Ibid, 206
- 42 A recent exposition of this line of argument is J.Tizard, 'Race and and I.Q.: The limits of probability', New behaviour, 24 April 1975 6 - 9.
- 43 For details of the debate between Yule and Pearson over the application of tetrachoric correlation, see D.MacKenzie, 'Pearson and Yule on the measurement of association: A case-study in scientific controversy', forthcoming in Social studies of science.
- 44 Op.cit. (note 27)
- 45 David Heron, Studies in national deterioration, Galton Eugenics Laboratory (1906)
- 46 Ibid.
- 47 Ibid.
- 48 Ibid.
- 49 Ibid.
- 50 See also H.Laski, 'The scope of eugenics', Westminster review, 174 (1910), 25 - 34, and, Kingsley Martin, Harold Laski A biographical memoir, London (1953), 8 - 12.
- 51 Karl Pearson, Nature and nurture. The problem of the future, (Eugenics Laboratory), London (1910), 4.

- 53 To see how seriously these concerns were taken as late on as 1948, it is worth consulting the Report of the Royal Commission on Population of 1949. Chapter 15, for example, addresses the likely consequences of 'differential fertility'. The need for research was stressed.
- 54 The interaction between Pearson and the Cambridge economists has been studied by Farrall (op.cit. (note 2)), who gives a whole chapter to the matter. Keynes and Marshall seem to have been anxious to establish a neo-LaMarckian position, showing that alcohol could affect germ plasms, and thereby make the children of alcoholics less worthy than would have been the case were their parents sober.
- 55 See L.Thompson, 'Origins and development of the M.R.C.', British Medical journal, No 2 (1963), 1290.
- 56 Karl Pearson, 'Tuberculosis, heredity and environment', Eugenics Laboratory lecture series , 8 (1911), 38.
- 57 Ibid , 46.
- 58 See, e.g., T.McKeown, The modern rise of population, London (1976), 83 - 85 and 153 - 162.
- 59 For a good general review of the various areas treated by the academic eugenisists and of the styles of argument which they deployed, see Edgar Schuster, Eugenics, London (1912).
- 60 See Charles Goring, The English convict (Abridged edn.), London H.M.S.O (1919).
- 62 See the Sixth Annual Report of the Eugenics Education Society (1913 - 1914).
- 61 See op.cit. (note 13).
- 63 A comparison of membership lists for the Eugenics Society and for the Sociological Society in Edwardian Britain shows an interesting overlap.
- 64 P.Abrams, The origins of British sociology, 1834 - 1914, Chicago, (1968)
- 65 R.H.Lock, Recent progress in the study of variation, heredity and evolution, 2nd. edn. , London (1909). See his chapter 10 'Eugenics'.
- 66 R.C.Punnett, 'Genetics and eugenics', in op.cit. (note 13), vol.1, 137 - 140.
- 67 W.Bateson, Biological fact and the structure of society, reproduced in B.Bateson, William Bateson F.R.S., Cambridge (1928), 353.

- 68 W.Bateson, op.cit. (note 67), 375 - 376.
- 69 For a brief account of Fisher's life and work, see ed. J.H. Bennett, Collected papers of R.A.Fisher, vol.1, Adelaide (1971). See pp. 23 - 52 for a biography by F.Yates and K.Mather.
- 70 R.A.Fisher, 'The correlations between relatives on the supposition of Mendelian inheritance', Transactions of the Royal Society of Edinburgh, 52 (1918), 399 - 433
- 71 R.A.Fisher, The genetical theory of natural selection, 2nd. revised edn., Dover Publications , New York (1958).
- 72 See, in particular, Fisher's 'Some hopes of a eugenicist', Eugenics review, 5 (1914), 309 - 314..For a bibliography of his eugenic writings, see Bennett, op.cit. (note 69), 11 - 22. See, in particular, Fisher's paper 'Positive eugenics', Eugenics review, 9 (1917), 206 -212 in which he argues the eugenic case for enhanced opportunities for members of the professional middle classes.
- 73 For details, see B.Norton and E.S.Pearson, 'A note on the background to, and refereeing of R.A.Fisher's 1918 paper 'On the correlation between relatives on the supposition of Mendelian inheritance', Notes and records of the Royal Society, 31 (1976), 151 - 162.
- 74 See Scarle, op.cit. (note 1), 83.
- 75 See e.g., Fisher, op.cit. (note 72).
- 76 Norton and Pearson, op.cit. (note 73)
- 77 R.A.Fisher, 'The causes of human variability', Eugenics review, 10 (1918), 213 - 220.
- 78 See e.g., Karl Pearson, 'Nature and nurture, the problem of the future', Eugenics Laboratory lecture series, 9 (1910).
- 79 Ibid, 27.
- 80 A simple modern guide to Fisher may be found in L.L.Cavalli - Sforza and W.F.Bodmer, The genetics of human populations, San Francisco (1971). See chapter 9.
- 81 See R.A.Fisher, op.cit. (note 70), 435.
- 82 R.A.Fisher, op.cit. (note 77), 214 -215.
- 83 See L.Hogben, 'The limits of applicability of correlation technique in human genetics', Journal of genetics, 27 (1933), 379 - 406.
- 84 See. e.g., R.Lewontin, 'Race and intelligence', Bulletin of the atomic scientists, March 1970, 2 - 3, and A.Jensen, 'Race and the genetics of intelligence', Bulletin of the atomic scientists, May 1970, 17 - 23. For Hogben's response to the eugenics movement, see also L.Hogben, Genetic principles in medicine and social science, London (1931), ch.8, 'The social applications of genetic principles'.

Chapter 8. The growth of Galtonian psychology: Intelligence objectively defined and measured.

Introduction

We have seen that Galton invited the formation of a new psychological discipline - a discipline of 'differential psychology' that would address issues such as the nature of intelligence, the relation of intelligence to simple mental functions that were easily tested, and, above all the questions of the social distribution of intelligence and of heritability.¹ We have seen also, in the previous chapter, that Karl Pearson and his biometric workers did a certain amount of work on the heritability of intelligence, work that embodied certain rather dubious assumptions about the correlation of intelligence with class.²

It transpires that these psychological interests were taken up by Britain's professional psychologists in the years prior to World War I, and, moreover, that the psychologists who entered this field were identical with the psychologists that studied and adopted the biometric methods developed by Pearson's laboratory. In fact, it does rather seem as if psychologists took up biometric methods at about the same time as the biological community was rejecting them - and this is a feature which persists to the present, for the correlation coefficient (for example) seems still to enjoy a place in the psychological literature which it never has achieved in the literature of biology.³

This being so, at least two interesting questions arise. One wonders whether, in adopting Pearson's statistical methods, these psychologists also adopted the distinctive philosophy of science which, we have seen, Pearson's statistical methods were intended to encapsulate. Secondly, one wonders whether the undoubted eugenic interests of these psychologists directed and shaped their work, and, above all, whether it ever distorted it - in the sense of leading them to claim levels of scientific support for their ideas which were grossly out of line with justified levels. This, of course, is an awkward matter to assess, but it does seem reasonable to allege excessive ideological bias if one detects either (a), the deployment of crucial arguments containing numbing non-sequiturs, or (b), the suppression or systematic doctoring of evidence.

Early days in British differential psychology

In 1900, Britain did not have a large body of professional psychologists employed in government or private institutions, or in universities.⁴ When the British Psychological Society was formed in 1901 at a meeting

of 'a new breed of psychological specialists working in laboratories, which, however small, possessed an identity of their own', there existed a mere handful of university posts. It was only in 1897 that the Cambridge University psychological laboratories came into existence, and, it was in the same year that Professor Sully persuaded the authorities at University College London to start a psychological laboratory in London - where, of course, Croom-Robertson had long held the Grote chair of mind and logic: what was new was the idea of psychology as an empirical and experimental science to be clearly separated from the less empirical pursuits of philosophy. The new psychology represented a break from the old anti-empirical view, which had linked psychology with philosophy and had led the Cambridge authorities to turn down Venn's and Ward's proposal for a laboratory in 1877 on the grounds that the psychophysical investigations they intended to pursue therein would 'insult religion by putting the human soul on a pair of scales'.⁵

Now, in a work of this nature, a long review of the personnel and institutions of British psychology would be out of place, doubly so as the existence of Professor Hearnshaw's Short history of British psychology, makes such a review redundant.⁶ What I plan to do is to consider the work of the handful of psychologists who, in one way or another, took up Galton's problem using Pearson's methods, and to use these analyses to answer, for a limited sample, the questions raised above. I shall focus centrally upon the work or influence of three men; Charles Spearman (1863-1945),⁷ William MacDougall (1871-1938)⁸ and Sir Cyril Burt (1883-1971).⁹ Others whose work will be mentioned are William Brown, the pioneer psychometrician, and Sir Godfrey Thomson, the distinguished developer of mental testing and of the methodology of factor analysis.¹⁰

Charles Spearman and the doctrine of 'g'.

It is fitting that we commence with Charles Spearman who employed Pearson's biometrical methods in work which argued that there was a very real thing which tests of intelligence measured namely 'g', or, as it was sometimes put, a 'central factor' in intelligence. Spearman, with some justification, was able to claim, from 1904 onwards, that his conception gave the answer to the question of what it was that the Frenchman Binet's¹²

'hotchpot' of tests really measured, claiming, in so doing, that the batteries of tests invented by this distinguished man in response to a call from the minister of public instruction anxious to find good ways of diagnosing those in need of special schools, measured 'g' itself, but more by luck¹³ than by judgement. The concept of 'g' has had a long life. Eysenck speaks most favourably of it in the Inequality of man, where his review of its various defences and of the modifications it has undergone, leads him to compare Spearman¹⁴ with John Dalton. Both men, says Eysenck, fathered extremely important conceptions. In both cases the original conception has been changed, but the credit accruing to the authors of the conceptions remains nonetheless. Interestingly too, in his recent 'rational reconstruction' of the nature-nurture debate as to the hereditary or environmental origins of intelligence, Peter Urbach isolates as one of the key claims of the hereditarians the proposition that 'all individuals possess a general mental capacity called "general intelligence" which enters with some (and varying) degree into all the diverse types of cognitive activity'.¹⁵ Finally, in this general context, it may be mentioned that Spearman was to hold the first chair of psychology at University College (see below for details) and to be succeeded by Sir Cyril Burt. We shall see that Burt made his early reputation by writing papers that focused upon Spearman's notion of 'g'.

Spearman, then, is a most fascinating subject. He fascinates in his own right, but also by virtue of his use of biometric methods and his interest in intelligence. He is the great early thinker about the nature of intelligence, and we can learn a great deal by examining his life and work, especially in the light of the two central questions posed in the introduction to this chapter. In discussing Spearman, I shall ask to what extent his adoption of biometric methods led him to adopt Pearson's neo-Machian philosophy of science and to what extent his work on intelligence was directed by his eugenic persuasions, indicated as they were by his membership of the Eugenics Education Society and by his writing for the Eugenics review.

The theory of 'g'.

Before proceeding to discuss the conditions of production of the theory of 'g' and the interpretation which Spearman put upon it, it seems wise to review the theory itself. This, roughly speaking, was a theory about the nature of human mental ability, though it had several

refinements. Spearman wished by his work to overthrow three views¹⁶ of mental ability which he considered to be widely held. They were what he termed the 'monarchic', the 'oligarchic' and the 'anarchic' views. The monarchic view held that every individual was possessed of a distinct level of an unitary intelligence, which entered with equal force into whatever he was engaged upon. It was the sort of doctrine which led men to declare that, had he been so directed, Newton could have produced drama the equivalent of Shakespeare's, and vice versa. It found reflection in Carlyle's claim that 'Odin, Luther, Johnson, Burns: I hope to make it appear that these are all originally of one stuff, that only by the world's recognition of them, and the shapes they assume are they so immeasurably diverse'. The oligarchic view held that there were several independent ability factors or 'basal powers' - such as, for example, reasoning, memory, judgement, discrimination etc. It was held that if an individual had a good judgement, then the judgement would be good wherever it was applied: but the claim of independence also asserted that an individual's possession or endowment of a good judgement gave no clue whatsoever to the level of, say, his memory. The anarchic view held that there were a very great number of independent factors, and that each of these developed in the individual quite independent of all the others.

In place of these three perspectives, Spearman wished to put a two factor theory of mental ability. If the political analogies are to be further extended, then one might say that Spearman, in matters constitutional, was a constitutional monarchist. For, as we shall see, his view was one of tempered monarchism.

This perspective upon things was first presented in 1904,¹⁷ in the American journal of psychology. This, at the time, was the obvious place to publish, as the British journal of psychology had hardly begun - it was the premier anglophone journal. There, Spearman sought to establish a theorem of intellectual unity,¹⁸ expressed as follows:

all branches of intellectual activities have in common one fundamental function...whereas the remaining or specific elements of the activity seem in every case to be wholly different from that in all the others. The relative influence of the general to the specific function varies in the ten departments here investigated from 15:1 to 1:4.

What was claimed was that all of one's mental processes involved a central unitary factor 'g', which was taken up in all mental activities and tasks to some degree. Each activity also involved a specific ability, unique to that activity. Thus, the key notion was as follows: there was a central factor 'g' entering into all mental activities. But the level of performance manifested by the individual in any test depended not only upon the level of his possession of 'g', but also upon the level of his possession of the specific factor unique to the activity in hand. Different tasks depended upon 'g' to differing degrees, with highly intellectual activities, such as mathematics and classics, showing very heavy dependence.¹⁹ Moreover, the level of the central factor could vary from individual to individual, and could be determined by giving suitable tests.²⁰ The 'hotchpot' nature of the Binet tests, Spearman claimed, had the beneficial effect that advantages due to possession of a high level of some specific abilities were cancelled out by the subjects' having to perform also in tests for which their specific abilities were low. A rough analogy to all of this might be obtained by supposing that, in respect of physical performance, it might be possible to isolate a factor of 'general strength'. Then we might find that some athletic performances were highly governed by the individual's level of general strength whereas others reflected it less closely, and involved to various degrees the possession of special faculties independent of general strength. Possibly, mountain walking might reflect general strength rather closely, while shooting might reflect it rather little, being dependent upon, say, the specific ability of good eyesight to some large degree. Once again, men might be expected to differ in the degree to which they possessed the factor of general strength.

(a) Hierarchies of correlations.

In the table below (table 1) we see the intercorrelations obtained between the scores of the members of a group of students in a number of tests set up or monitored by Spearman. The tests, as [^]my be seen, range from mathematics to sensory discrimination, and the various figures give the correlation coefficients connecting the scores of a group of subjects in the various tests. Thus, for example, the figure 0.83 gives the measure of correlation between performance in classics and performance in French. The data, it should be noted, were 'corrected', using the methods developed by Spearman for dealing with heterogeneous material.²¹ Now, if we suppose, as did Speannan,

Image removed due to third party copyright

Table 1. Based on Spearman's 1904 paper, as reproduced in Brown and Thomson's Essentials of mental measurement.

Image removed due to third party copyright

Fig.1. Taken from Spearman's 1912 Eugenics review paper, 'The heredity of abilities'.

that test results are determined by two factors - by a 'general' factor, tested in different degrees by different tests - and by a factor specific to the test in hand, and that the distribution of specifics is an independent one (i.e. that an individual's having one specific in good measure is no guide as to the degree of his possession of any other factor) then we would expect just the hierarchy of test scores here depicted. That is to say, the correlations may be arranged so that they form a table everywhere diminishing as we move from left to right, and from top to bottom. Moreover, if we take any two adjacent columns, or, any two adjacent rows, we find that the ratio between adjacent elements is always the same for all pairs of elements in any two columns or any two rows. These expectations can easily be derived from the theoretical assumptions by using the calculus of partial correlation,²² notably as developed by Pearson's one-time pupil, George Udny Yule (1871 - 1951). The derivation is given in the appendix to the chapter.

This expectation of a hierarchy was the main observable prediction of the theory: the issue of whether data did give a genuine hierarchy, and, not less than that, the issue of whether there weren't other theories that predicted the same result, were ones that covered the pages of psychological journals for many years.²³ In particular, it is worth noting Godfrey Thomson's assertion that a hierarchy was predicted by a variant of an 'anarchic' theory of mind. Thomson was so enamoured of this point that he continued to develop it even in his Factorial analysis of human ability.²⁴

The interpretation of 'g'. (a) In general.

At first, one might suppose that 'g' was intended by Spearman just as a statistical abstraction, perhaps owing its importance to the predictive utility of measures of a person's 'g', and to the applicability of the same in a range of administrative technologies. Spearman, and his students after all,²⁵ did do a great deal of mental testing. But, this would be a very circumscribed view of 'g'. For, when we look at the way in which Spearman first introduced 'g' in his 1904 paper, and at his later writings on the subject,²⁶ including a twenty-year debate with Godfrey Thomson, it becomes clear that at no time did Spearman ever subscribe to an instrumentalist philosophy of science. Rather, it seems that 'g' found a rich interpretation in an interesting, though unclear, theory of mind and brain. It seems likely too that Spearman's coming to work on 'g' and his way of developing the notion may not be properly understood in isolation from his personal

ethical beliefs, or from his participation in some of the strong ideological currents of his period. The remainder of the section is devoted to discussing the interpretation and historical genesis of 'g'.

(b) Ethical relations and interpretation

To understand the interpretation of 'g' and its genesis, it is useful to start with a consideration of what is known of Spearman's early development. His papers have not been located, and we have therefore to go upon what he himself recorded in his brief autobiographical article. He was the product of the English upper middle classes, and, on leaving school, went into the army as he was undecided as to what career he should follow. The army, presumably, was thought a suitable destination for one so afflicted. Spearman was an officer who went on to staff college, and doubtless received a good education. He remained a soldier until 1897, when he went to Leipzig to study with Wundt, the pioneer of experimental psychology. For his military years, which he considered wasted, Spearman was to write that he mourned 'as bitterly as Tiberius ever did for his lost legions'. His spell in Germany was interrupted by the outbreak of the Boer War, and he returned to act as staff officer in Guernsey for the duration. In 1902, he took his Ph.D. in psychology from Leipzig, and stayed on in Germany until 1906, studying brain physiology, but finding its attacks on the mind-problem unsatisfactory. In 1907 he was appointed reader in psychology at University College London, in succession to William MacDougall. And, in 1911, on the retirement of Carveth Read, he took the professorship. In 1928, he was able to separate psychology from philosophy at University College, which he did with a view to integrating psychology with other empirical scientific disciplines. At University College, he had a series of distinguished pupils and was able to achieve for his London school a dominant place in British psychology. Finally, he was succeeded by Cyril Burt.

The obvious question raised by Spearman's career is that of why, in 1897, the military man turned psychology student. This he did at a period when there were almost no posts in psychology, and certainly the number of academic posts in Britain could be counted on the fingers of one hand. A first answer to this question appears to lie in Spearman's self-confessed early and secret devotion to philosophy. This went back to his tenth year,

becoming acute in his fifteenth year. The precise nature of these thoughts is unrecorded, but Spearman records that they were generally accompanied by an 'intense stirring in an ethical direction such as highly emotional yearnings for the good of all mankind'. In the army, Spearman continued with private readings in philosophy, becoming convinced, as he put it, that if ever a genuine advance was to be made in philosophy it would come mainly by way of psychology. And, starting upon psychological readings, he encountered at first the works of Locke, the Mills and Bain - generally described as associationist philosophers. To this school he reacted violently, finding their ideas crude and erroneous. But, this was not all, for, he wrote;²⁷

My conviction was accompanied by an emotional heat which cannot, I now think, be explained on purely intellectual grounds. The main source of this heat I take to have been - little as I admitted this at the time - of an ethical nature. Sensualism and associationism tend strongly to go with hedonism, and this latter was (and is) to me an abomination.

Knowledge of these feelings towards associationism gives some help when trying to understand Spearman's work. We find that he responded to associationism with a strong desire to construct psychology anew by producing principles or laws²⁸ which would do for psychology what Newton's laws did for physics. The result of this desire was the production of three 'noegenetic' laws which allegedly described the processes whereby the mind could generate new items of knowledge through an active power of perceiving new relations between items given in experience. History has not dealt kindly with these laws. The leading historian of British psychology has written thus:²⁹

Spearman's noegenetic scheme was not without its points. It recognised the creative potential of mind, which had often been neglected; at a time when the 'Gostait' ideas were becoming fashionable, it kept alive the analytical approach; it proved of some value in intelligence test construction. But as anything like a definitive and final account of cognition it was hopelessly jejune.

The precise nature of these laws need not detain us, but, being laws expressing the active power of the mind, they appear to put Spearman into the same intellectual camp as men like Stout and Ward, who, in a general reaction against the 19th century doctrines of scientific naturalism, attacked associationist psychology by insisting that mental life was a far more active

and creative process than associationism had allowed.³⁰ This, perhaps, was a fairly characteristic feature of the continental psychology to which Spearman had been exposed, a psychology, that is, which had strong Kantian influences.³¹ But, in Britain by all accounts, it was a recent departure, whose beginnings were perhaps most clearly marked by Ward's famous Encyclopedia Britannica ^{article.} of 1885. William MacDougall, another refugee from associationism noted this trend in Spearman's psychology when, in his Energies of men, he wrote that³²

Spearman repudiates the tendency to make psychology wholly subordinate to, a mere branch of, mechanistic physiology.

Spearman has forcibly reminded us that mental life is an active process; that mental activity involves, fundamentally and in all its phases, the grasping of relations, relations of time and place and causation, of likeness and difference, of magnitude and intensity and quality, and many others; that any account of our mental life which does not fully and frankly recognise this activity is very inadequate and misleading.

Spearman believed in an active, synthesising mind, and eventually felt able to bring together his work on 'g' with his work on noegenetic laws. This he did by identifying 'g' as the individual's power or energy for carrying out noegenetic synthesis.³³ It was perhaps a measurement of the 'horsepower' of the individual's active mind. The public utterance of this identification came fairly late in Spearman's career, but it does seem pretty clear that something of this sort had been in his thoughts from the earliest phase of his work. For, other points apart, we find that, from an early stage, 'g' is identified as a measure of mental energy.³⁴ Everywhere in Spearman's ~~work~~ ^{work} we find the idea of 'g' as standing for a level of energy, energy which, he seems to have thought, would turn out to have some sort of physiological reality, though he was at pains to stress that,³⁵

there is no reason why such energy should have more than a broad analogy to anything of the kind that has been suggested hitherto.

Simultaneously he was not ultimately much troubled at the prospect of being unable to find a suitable physiological energy in the brain, for,³⁶

should the worst arrive and the required physiological explanation remain to the end undiscoverable, the mental facts will nonetheless remain facts still. If they are such as to be best explained by the concept of an underlying energy....it will have to be regarded as purely mental.

And, his hope, all along, appears to have been for a model which employed an engineering analogy, and which depicted the cortex of the brain in each individual as having a certain constant energy level, to be identified with the level of 'g'. But, within the cortex, there would be shown to exist certain groups of neurons, the analogues of engines, which would take up the general energy of the cortex when a specific function was embarked upon. Here then was a possible physiological model for the two-factor system. The general energy of the cortex stood for 'g' and the efficiency of the specific engines measured the individual's specific abilities. High performance in any test could be due to a good 'g' and an averagely efficient engine specific to the task, or, it might be due to an average 'g' and a particularly efficient specific engine. Mental testing would determine which.

The full range of Spearman's thoughts in this area cannot be dealt with briefly. But, it is well to note that, in talking about energy, he was far from being completely out of step with the general trends of psychology at his period. There is, to take a famous example, the case of Freud whose unpublished Project for a scientific psychology,³⁷ written in 1895, deployed a hypothetical model of the mind in which a great range of important mental events were interpreted in terms of the flow of energy between neurons. Freud too was vague, deliberately, about the possible nature of this energy, thinking it probably similar to chemical or to electrical energy, but referring to it, not as 'g', but as 'Q'. A quick perusal of Spearman's works reveals references to a great many writers who discussed the possibility of mental energy and its possible physiological bases, including, for example, Lehmann, whose thoughts on the possible interconvertibility of mental and bodily (physical) energy were taken very seriously by Spearman.³⁸ More generally, we find frequent references to 'energy' in Galton's works, which, we know, Spearman read most avidly - (though Galton, perhaps, had in mind a less technical conception than, say Freud). Moreover, if we look to Spearman's predecessor at University College, William MacDougall, a one-time student of Sherrington, we find him, in 1902, attempting to explain various phenomena of attention and consciousness with a model that allowed for 'neurin', a hypothetical form of energy, conceived on the analogy of a fluid, which, in the brain, flowed from areas of high to areas of low potential.³⁹ Differences in temperament, for example, were seen by MacDougall as connected with differences in synaptic resistance to the flow of neurin. Perhaps it is not surprising then, that MacDougall would later write a book entitled The energies of men, and, in line with Spearman's general orientation,

we find in MacDougall (another opponent of associationist psychology), a much more aggressive critic than Spearman, who took his criticisms further, and could be found writing that the observed unity of consciousness was,⁴⁰

a unity of a unique kind which has not analogue in the physical realm, and that cannot properly be regarded as consisting of elements, units or atoms of consciousness, put together or compounded in any way; consciousness cannot, therefore, parallel anything in the nervous system, which is composed of discrete units; it must be something sui generis.

Spearman, then, it would seem, in company with many other psychologists of his period, was not at all favourably inclined towards the instrumentalist philosophy of science promoted by Pearson, and around which Pearson's statistical methods had been built. 'g' had a very rich and non-positivistic interpretation in the eyes of its author. Certainly, current texts seem to ignore this aspect of Spearman's thought, and, in these books he comes across as a pioneering factor analyst, a man searching for statistical factors capable of serving as effective instruments of prediction and data summarisation.⁴¹ He also appears to be much closer in spirit to Pearson's ideas^{as depicted in such books} than closer inspection reveals to have been the case.

(c) The politics of intelligence.

So far only one component of the social relations of 'g' has been dwelt upon. But were one to ask what it was that made the conception a thing of value to its author and to its supporters at the time, then a full answer would have to review not only the factors already seen, but also the strong stream of eugenic ideology which flowed briskly in England in the first years of the 20th century.

The father of the eugenics movement, as we have seen, was Galton himself, whose work articulated a thoroughgoing biological interpretation of social phenomena by stressing the power of nature over nurture, and by arguing, with some empirical backing, that one could assess people for their overall levels of 'natural ability' or 'civic worth', and that these levels were determined by heredity rather than by environment.⁴² He was in a strong sense the father of differential psychology, one of the first to conduct systematic testing of large numbers of people with a view to finding

the differences in faculties that persisted in the population. Intriguingly, the American pioneer of differential psychology James McKeen Cattell, found favour with Galton after being told by the pioneering German psychophysicist Wundt that his ambitions for a differential psychology⁴³ were ganz Americanische. The continental tradition represented by Wundt, had less interest in the differences between men, and sought instead for the universal features of mind. As we have seen, in Galton's thought, class differences tended easily to become interpreted as hereditary differences in 'natural ability', and this was an ever-present tendency among the membership of the Eugenics Education Society, which, with its predominantly professional membership, had a particular interest in that which gave the professional man his status and claim to position in society - i.e., not land or capital, but skill, training and educated ability. Among the membership, as we have seen, there was a tremendous interest in the fortunes of this able class, and frequently expressed fears that it might be declining proportionately in the face of superior reproductive efficiency on the part of the lower classes. Spearman fits in well with this general pattern of concerns, which, it should be noted, inclined other psychologists such as MacDougall and Burt to join the society. For, we find that, as an enthusiastic reader of Galton's works - notably of Galton's Hereditary genius of 1869 - he and his wife became members of the Eugenics Society before the first war. Now, within the society there were, of course, many shades of opinion, both as to theory and to preferred policies, and we have no reason to suppose that Spearman would have espoused the hard-line views of, say, a Major Leonard Darwin - who denied⁴⁴ that anything other than wealth could serve as a guide to a man's genetic worth - or of a William Dampier Whetham (the future Sir William Dampier) who would restrict opportunities to the lower classes in order to give the professional classes the confidence to breed freely, in the knowledge that positions would be available for their offspring.⁴⁵ However, it is quite clear that a significant source of the value with which Spearman invested his work came from what he perceived as its social implications and applications, and, above all, from its possible eugenic connections.

The sort of problem which he was pleased to claim that his work resolved is that posed in humorous form in the cartoon reproduced ^{above}~~below~~. (Fig.1) The cartoon is taken from Spearman's 1912 paper on the 'Heredity of abilities and was one he borrowed from the Daily Mirror (then, as now, a low-brow and popular paper).⁴⁶ The cartoon, whose location gives an indication of the fame which eugenics achieved in Edwardian England, poses the classic problem for

eugenists. For, having abandoned Spencer's view that only the environment should select in favour of selection by committee, the eugenists were faced with the problem of choosing what features they thought were desirable enough to be bred for. As the members of the eugenics society were a fairly well-integrated group, it seems unlikely that the issue of choice was one that could split them asunder, but, at all events, an 'objective' criterion was required where their opinions to be presented as more than sectional interest. Spearman seems to have considered that, by his authoring of 'g', he had solved the problem - for 'g', which, it will be recalled, was supposed to exist in differing degrees in different individuals, provided a single objective scale and a unidimensional one at that - on which everyone could be compared. Moreover, as he wrote in another paper, in the British journal of psychology, the concept of 'g' and the possibility of objective testing which it raised, made it seem,⁴⁹

not altogether chimaeric to look forward to the time when citizens, instead of choosing their career at almost blind hazard, will undertake just the professions really suited to their capacities. One can even conceive the establishment of a minimum index to qualify for parliamentary vote, and, above all, for the right to have offspring.

Spearman, over many years, was ardent in the defence of his theory of 'g'. This theory, or the 'theory of two factors' as it is sometimes known, was the subject of a vast and extended debate, particularly between Spearman and Sir Godfrey Thomson, the noted psychometrician. Thomson was particularly anxious to show that Spearman's demonstration of the existence of a hierarchy amongst test scores could not be taken as strongly confirmatory of his two-factor theory, because it was a simple matter to show that a rival 'anarchic' theory of mind led to exactly the same prediction. If one assumed that the mind contained an indefinitely large number of independent units or 'bonds', and that different tests represented random samples of these bonds, albeit of different sizes, then the laws of probability alone were sufficient to guarantee the production of a hierarchy. There was no need to suppose, along with Spearman, that there was such a thing as a general factor. Now, the debate between Thomson and Spearman went on for many years. The literature generated in this debate was massive, and, it is worth noting that when, in 1931, Helen M. Walker published her Studies in the history of statistical method, she felt obliged to include a chapter on 'The theory of two factors'. Possibly she felt so obliged as she was a professor of education at Teachers College, University of Columbia.⁵⁰ And, as the subject was long, and slightly out of line with the remainder of her book, she simply gave a useful annotated

bibliography of forty-two items, which, in large measure, recount the struggle between Thomson's 'sampling theory' and Spearman's 'two-factor' theory.

In her introduction, Walker suggested that Spearman's theory was one of the most striking illustrations of an educational and psychological hypothesis which has been defended and attacked almost solely by statistical arguments. This was no doubt true, but what is overlooked is the source of Spearman's furious defence of his theory. For, why should he be so anxious to defend at such length a theory which, in Popperian terms was rather a poor one - for, as we have seen, it made just one real prediction, also made by the rival theory?⁵¹ The answer to this riddle, doubtless, lies in the rich interpretation which Spearman gave to his conception, in the central role that it played in a psychology in which (in Spearman's opinion) the entire range of all cognition whatsoever, as regards both form and material, would appear to receive its definite and final boundaries, and in its place in Spearman's and other's eugenic and social-reformist thinking. Spearman could not let his concept of 'g' pass into extinction without at the same time jeopardising the continued viability of these other, distinctly non-statistical aspects of his thought and work. He was, it should now be obvious, a great deal more than the prototypic factor-analyst that holds his name in so many modern texts.

In summary then, we can see that Spearman saw some of the value of his work as residing in its power to resolve eugenic problems. There is no denying that he was a member of the Eugenics Education Society and that he wrote for its organ the Eugenics review. In these writings we find him stressing that the theory of 'g'⁵²

appears to make possible, for the first time, meaningful and reliable mental measurements, a matter in which previous researches have been gravely defective.

Therein, we find him arriving also at 'a conclusion of fundamental importance for eugenics', namely that⁵³

though unquestionably the development of specific abilities is in large measure dependent upon environmental influences, that of general ability is almost wholly governed by heredity.

But, we find very little of such writing in Spearman's work, all of which suggests strongly that he was much more impressed by the power of his theory to resolve what he took to be outstanding problems in the psychology of

cognition than by its eugenic potential. This certainly is a view which is consistent with the reminiscences of Stevenson,⁵⁴ a pupil of Spearman's, who recalls that when Spearman was succeeded by Cyril Burt at University College London in 1931 there was a feeling of 'let down' among some psychologists as Spearman was considered to be 'theoretical' and 'scientific' in a 'pure' sense, whereas Burt was perceived as 'practical' and as 'applied'. Indeed, if there is one area of possible 'bias' in Spearman's work, one suspects that it may lie in overstrong claims for the proven existence of the clean hierarchy of test score correlations implied by the theory of 'g'. We have seen after all that the results published had been subjected to correction: possibly there is room for a careful consideration of Spearman's claims in this direction, but, given the remarks above, it seems probable that any 'irregularity' along these lines should be ascribed to metaphysical rather than to eugenic zeal.

Most interestingly, Spearman's adoption of Pearson's biometric methods did not connote an approach to science based upon the positivistic philosophy which Pearson had intended his biometric methods to encapsulate. In Spearman's work, Pearsonian methods were deployed to support ideas of 'psychic energy' - ideas for which there was no place in Pearson's philosophy.

At this stage, I would like to leave the case of Charles Spearman and go on to those of William MacDougall and, especially, Cyril Burt. In my treatment of Spearman I have, of course, mentioned his twenty-year debate over the theory of 'g' with Sir Godfrey Thomson. It seems appropriate that some further explanation of this debate should appear, and though this chapter is not the place for a lengthy analysis of the debate, it does seem to me that there is room for a brief sketch of Thomson's position, which, accordingly, is contained in the appendix to the chapter where a variant version of Udny Yule's exposition of the manner in which the theory of 'g' leads to the expectation of hierarchy also finds a place. For the present, however, let us look briefly at the early career of Cyril Burt.

William MacDougall, Cyril Burt and the psychology of eugenics.

No English psychologist, present or past, enjoys the renown of Cyril Burt, whose work has recently been heavily criticised by Leon Kamin, in his polemical Science and politics of IQ which shows convincingly that Burt's⁵⁵ published work on twin-studies contains improper data. Burt is known as a hard-line hereditarian, as a man who desired, as strongly as Galton ever did, to show that in matters of intelligence, that heredity dominated over environ-

ment. Indeed, the general outline of his life's work has been neatly summarised by his biographer L.S. Hearnshaw, who insisted that:⁵⁶

His work can be regarded as a working out of the programme, first envisaged by Francis Galton, for a psychology of talent and character, rooted in evolutionary biology and genetics, and recognising the importance of individual differences, and quantitatively based. Towards the establishment and application of such a psychology Burt worked with undeviating consistency. There is a single thread of purpose uniting his first publication in 1909 and his last posthumous papers published in 1972.

Clearly, in what remains of this chapter, I cannot discuss the whole of Burt's career as a psychologist, but, as a man who employed biometric techniques in the pursuit of a Galtonian psychology in the period prior to the first war, he is a natural object of attention.

Cyril Burt was the son of a doctor who, interestingly, had a great regard for Francis Galton. The young Burt was dependent on scholarship successes and entered a leading public school on the basis of examination performance, thereafter running away with the school prizes. Later he won a further scholarship to Jesus College Oxford, where he took classics - basically because no science had been taught in his school. At Oxford he took only an average degree, possibly because he had given his heart to the study of psychology under William MacDougall, then precariously situated in Oxford as Wilde reader in psychology. At Oxford, Burt came into contact with Karl Pearson who came to talk on 'correlations and lines of closest fit', here too, he first encountered Biometrika.⁵⁷

On graduating, Burt was awarded the John Locke scholarship, which enabled him to work abroad, and, in 1909, he was appointed lecturer in psychology in the University of Liverpool⁵⁸ in the department of physiology then run by Charles Sherrington. To this he held until 1912, when he

he took up the post of educational psychologist to the London County Council - the first post of its kind ever. In 1924 he was appointed professor of education in the University of London, and, in 1931 he succeeded Spearman as professor of psychology at University College, becoming the first psychologist to be knighted in 1946.

The psychological work which established Burt as a leading figure⁵⁹ was his paper on 'Experimental tests of general intelligence', published in 1909, but on the basis of data which he appears to have collected whilst still the pupil of MacDougall in Oxford. It is on this early, career-establishing work that I wish to focus. It was this work, after all, which gave Burt the

first step up along the steep road to knighthood.

In the introduction to his work, Burt plainly acknowledged the debt he owed to Galton, whom he perceived as having first introduced the notion of 'general intelligence'. But, more to the point, he indicated that he hoped to show which tests would give the best estimate of a person's 'general intelligence', doing this in a series of experimental investigations carried out⁶⁰ upon boys from (a) an Oxford elementary school, and (b) an Oxford preparatory school, in 1907 and 1908 with the assistance of J.C. Flugel. The boys at the elementary school were of lower middle class origins, whereas the boys from the preparatory school were the scions of the Oxford professoriate and of other persons from equivalently ranking fractions of the professional middle classes.

In his research, Burt subjected the boys to series of tests which, in his view, represented

as far as possible the various main aspects and levels of mental process.

These included sensory tests, such as 'discrimination of two points upon the skin', motor tests such as tapping and card dealing tests - in which the subjects had to deal cards into five hands as quickly as possible -, sensori-motor tests of the alphabet - finding sort, association tests in which the subject had, for example, to trace over a pattern seen only in a mirror and, finally, tests of volutary attention in which the subjects had to show what rate they could accurately record simple information with which they were supplied.

Burt, of course, was interested in intelligence, and, as he put it⁶¹

286

To determine the degree to which the various tasks might be considered satisfactory tests of General Intelligence, it was necessary to obtain an independent estimate of the relative intelligence of the reagents tested. For this, recourse was had at the conclusion of the experimental part of the work to their headmasters, their teachers, and their schoolfellows, who undertook to draw upon on the basis of their general experience of the examinees independent lists, grading them in order of General Intelligence.

These lists were drawn up then, in the light of schoolmasterly experience, and then, by using Spearman's 'fool-rule' method⁶² of determining correlation, Burt was able to come up with figures indicating the degree of association between these estimates of relative intelligence and the rankings shown by the reagents in respect of the various tests.

Burt found that the level of correlation altered with the nature of the test, noting that⁶³

- Of the twelve tests, six furnish coefficients below .50 and six furnish coefficients above .50. The former six - the simple sensor and motor tests - are thus of little use in the empirical diagnosis of intelligence. Among the latter six, no single test, at any rate in its present form, can be claimed as a self-sufficient instrument for measuring and detecting ability in individuals. But they indicate the direction in which such a test may hopefully be sought. Particularly promising are the four new tests. Of these Mr McDougall's 'Dotting Machine' seems to be the most scientific.

(MacDougall's dotting machine was used in the attention tests). But, he claimed, ^{from} an amalgamated series of test results, gotten by 'making a grand average of the six gradings from those that give coefficients above 0.5 and arranging the boys accordingly', we obtain a list correlating with the headmaster's order to the extent of .85 at the elementary school and .91 at the preparate. Thus, he concluded, by means of half a dozen tests,

we are able independently to arrange a group of boys in an order of intelligence, which shall be decidedly more accurate than the order given by scholastic examinations, and probably more accurate than the order given by the master, based on personal intercourse during two or three years, and formulated with unusual labour, conscientiousness and care.

Here then, it seems, Burt was advancing the claims for a method of intelligence testing with no literary or cultural content, and, this done, he was able to develop a strong eugenic line. Previous workers, Burt argued, including Karl Pearson among this group, had failed to establish 'the growing belief

that the innate characters of the family are more potent in evolution than the acquired characters of the individual'. His work, however, seemed of a more decisive nature, for it indicated that the preparatory school boys gained superior marks in all tests bar the two tests of sensory discrimination based upon the discrimination of weight and of two point on the skin. Hence, Burt concluded .

wherever there are correlations with intelligence,
there (so far as we can discover) --

.. cont. over leaf.

boys of superior parentage are themselves superior. Moreover, at the sound, tapping, memory, mirror, alphabet, and dotting tests, the preparatory boys are superior even to the cleverest section of the elementary boys.

Now, argued Burt, since the lower middle class boys all enjoyed reasonable standards of nourishment and environment, it was implausible to attribute the observed differences between the two groups to such factors: nor could they be attributed to practice or to several other possible explanatory items. The conclusion was straight-forwardly Galtonian⁶⁴

We may conclude that the superior proficiency at intelligence tests on the part of the boys of superior parentage, was inborn. And thus we seem to have proved marked inheritability in the case of a mental character of the highest 'civic worth'.

Intelligence, in short, was shown to be 'inherited to a degree which few psychologists have hitherto legitimately ventured to maintain'.

This, then, was the general tack of Burt's first major piece of research, which, as we can see, was, ab initio designed so as to resolve the question of the relation between social class and innate intelligence. As it was the piece of research which propelled him into his long and influential career, and as it both employed biometric (or statistical) methods and helped develop the discipline of differential psychology, it is interesting to consider how and why Burt came to do this work.

The answer appears to lie in the connection with MacDougall, who, as we have seen, was Burt's first, Oxford, psychology tutor. Several sources seem to indicate a scenario in which MacDougall, alarmed by reports of growing fertility on the part of the lower orders, took steps to show the seriousness of this state of affairs by commissioning work intended to establish that class and genetic worth were indeed correlated. Evidence for this comes from at least three sources, of which two are articles by Burt and the third is MacDougall's work National welfare and national decay (1921).

In National welfare, MacDougall wrote of the proposition that⁶⁵

the social stratification which exists in modern industrial communities is positively correlated with a corresponding stratification of innate moral and intellectual quality.

that it was the assumption which lay at the root of 'eugenic propaganda', and that, by virtue of being an assumption, rather than a proven proposition, it gave eugenics its 'greatest weakness'. But, he also recalled,⁶⁶

two of my pupils (Mr C. Burt and Mr H.B. English) made the first contributions... toward filling the gap.

In Burt's own writings, the same account appears. In his Intelligence and fertility of 1952 he claimed that his work of 1909 had originated in a suggestion of William MacDougall, itself consequent upon Heron's work on birth-rate differentials⁶⁷

One of Karl Pearson's fellow-workers, David Heron, had recently published considerable evidence demonstrating that the birthrate differed widely with differences of social level, that of the professional classes being less than half that of the so-called working classes. It was therefore arranged that two groups of children, representing these two contrasted social classes, should be examined. Compelte age-groups (12.0 - 13.0 years) were chosen (i) from a well-known preparatory school (the Dragon School), where the pupils were sons of men eminent in the intellectual professions (bishops, professors, scientists, civil servants, etc.), and (ii) from an elementary school, where the parents were local tradespeople and working men, not so ill-paid, however, as to lead to serious handicaps from poverty or poor health.

In a paper of 1951,⁶⁸ Burt mentions also that there had been established, in 1905 a committee of the anthropological section of the British Association which 'drew up a scheme for a comprehensive survey, and a sub-committee was set up to consider the inclusion of psychological measurements'. MacDougall was the chairman of this sub-committee^{and, apparently, saw} himself as engaged upon a 'sociological survey of the nation's man power'.

So, it seems that there is fairly strong evidence to suggest that Cyril Burt's first entry into psychological research came in context of an attempt by the eugenicist MacDougall to counteract the 'weakness' in contemporary 'eugenic propaganda' - namely the circumstance that potentially alarming statistics, like those provided by David Heron, were based upon the mere assumption that the social order reflected an innate biological ordering of men in respect of inherent 'natural ability', 'civic worth' or what have you. If this assumption were not made or demonstrated, then all of Heron's warnings, as noted, would instantly fall to the ground.

In conclusion therefore, it would seem that MacDougall⁶⁹ was an active eugenicist keen to show the heritability of intelligence and to demonstrate the truth of the eugenic assumption that the social order reflected an underlying biological order. Burt, it seems clear, was commissioned to do this

work for him, and, on the strength of having established (i) a correlation between the rankings given by certain tests and by headmasterly estimates of intelligence amongst two groups of boys, and (ii) an overall higher level of marks on these tests amongst the scions of the upper professional classes, asserted the inborn superiority of the upper class boys. Thus he might be seen to repair the 'weakness' of eugenics, thereby, no doubt, gaining the good will of MacDougall, Galton and Pearson, whose views, Burt noted, were solicited when the question of appointing an educational psychologist to the London County Council was mooted.

The question of whether or not Burt accepted Pearson's philosophy of science when adopting his biometric methods is hard to settle from his early work, though later writings, particularly on the results of the application of factor analytic techniques, suggest not. The question of ideological bias in Burt's early work is perhaps the more interesting one, for, as we have seen, it was done in a welter of eugenic sentiments. One must, of course, be careful to avoid imputing to the early Burt the same sorts of excesses which, Kamin has alleged, are typical of his later period. But given the nature of Burt's evidence as provided in his 1909 paper, it can reasonably be suggested that the move to a categorical assertion of the innate superiority of the higher-class boys just is an assertion that is a great deal stronger than that demanded by the evidence or allowed by the structure of the argument. For, surely, from the fact that upper class boys do better in tests giving rankings closely resembling the headmasters' rankings (where each headmaster ranks only his own boys) is a thin premiss from which to advance to conclusions of innate superiority. It is a doubly interesting point as the 1909 paper marks the first step of a long career in which Burt sought, time and again, to show a cause and effect relation between IQ and class - with IQ far more a cause than an effect. This claim of a causal relation was to be very widely disseminated, and so, whatever the merits of the claim, we must surely learn to see the interaction of Heron, MacDougall and Burt before the first war as one that was possibly of the greatest importance in the formation of semi-popular thought on the nature of social class in Britain. I use the qualification 'possibly', because, as I have already noted in the case of Pearson, we cannot immediately infer from a view's wide dissemination to its having had a commensurate impact.

Conclusion

Insofar as any general conclusions may be drawn from the core of this chapter, they are as follow. Biometric methods seem to have been remarkably well-received among that section of the British psychological community that studied intelligence and the distribution of individual differences. In fact, whereas the biologists more or less rejected biometric methods - especially as illustrated by the various guises taken by the correlation coefficient - they were well-received amongst the psychologists, and were given prominent places in manuals of methodology, as, for example in the Essentials of mental measurement, first produced in 1911 by William Brown, a one-time student in Pearson's laboratory, and put through successive editions by Brown with the assistance of Godfrey Thomson. Such methods remain popular today, especially as illustrated by journals such as Psychometrika, though it is interesting to note the comment of Udny Yule, writing in 1921, that⁷⁰

Statistical methods, I say, should be regarded as ancillary, not essential. They are only essential where the subject of investigation is itself an aggregate, as a swarm of atoms, or a crowd. But there the subject is the individual, not the aggregate of individuals as such. This being the case, statistical methods are only necessary in so far as experiment fails to attain its ideal, the ideal of only permitting one causal circumstance to vary at a time. And it should always be the aim of the experimenter not to revel in statistical methods (when he does revel and not swear) but steadily to diminish, by continual improvement of his experimental methods, the necessity for their use and the influence they have on his conclusions. Statistical methods are not only ancillary; they are, to the experimenter, a warning of failure.

Yule, I have already noted, had already shown signs of deviating from Pearson's views when, in the early years of the 20th century, he staged an attack on Pearson's indiscriminate employment of the correlation coefficient. Don Mackenzie has shown that these reactions and others were in harmony with Yule's general outlook on life and society.

Secondly, it seems that in some instances, development of the Galtonian tradition of psychology was attended from the earliest moment by a high level of eugenic zeal - as in the case of Burt and MacDougall. But in other instances, as for example, in the case of Spearman and 'g', the zeal seems to

have been distinctly more philosophical. Certainly, it is worth pointing out that a fascination with biometric methods did not invariably go hand in hand with a predominantly hereditarian outlook, a point illustrated by the work and thought of Godfrey Thomson (some aspect of whose works are discoursed in the appendix to this chapter) and William Brown, Thomson's colleague who on one occasion ^{claimed} that Spearman's general factor was due to environmental conditions.

Appendix

In this appendix, I first offer a slightly modified version of Yule's simple demonstration that Spearman's theory does lead to the expectation of a hierarchy of the sort described in the text, and then, to illustrate the reference to Godfrey Thomson's sampling theory, I also offer a brief indication (but no rigorous discussion) of the way in which this rival theory containing no central factor also led to the expectation of an hierarchy. The whole Thomson-Spearman debate is a subject for separate treatment; here I can hope only to give a flavour of the pro positions.

1/ Spearman, the theory of 'g' and the consequent hierarchy.

Let us suppose that a and p are two mental tests - eg., sitting a latin exam - and that g is the general factor. Then, in Yule's notation, $r_{ap.g}$ is the correlation that would be observed between the test scores of people having equal g levels. This equals,

$$\frac{r_{ap} - r_{ag} r_{pg}}{\sqrt{(1 - r_{ag}^2)(1 - r_{pg}^2)}}$$

But, the independence of the specific abilities brought into play in tests a and p guarantees that the level of g is the sole cause of correlation between a group's scores in a and its scores in p. Hence, by Spearman's theory, the partial correlation $r_{ap.g}$ must take the value zero.

Hence, $r_{ap} = r_{ag} \cdot r_{pg}$

Similarly $r_{bp} = r_{bg} \cdot r_{pg}$

Hence

$$\frac{r_{ag}}{r_{bg}} = \frac{r_{ap}}{r_{bp}} = \frac{r_{ag}}{r_{bg}}$$

It is an obvious consequence that if the last equation ^{is} true of any four of the tests, then a 'hierarchical' arrangement of the correlations between scores must be possible, and, moreover that adjacent members of column i and column $(i+1)$ in such a hierarchy will always have the same ratio. Similarly with the rows.

(This derivation is based very closely upon that given in W. Brown and G. Thomson, Essentials of mental measurement, Cambridge, (1921), pp. (65 - 166).

2/ Thomson, the sampling theory and consequent hierarchy.

Thomson developed his sampling theory, which does not contain a central factor 'g' or anything like it from 1911 onwards. The version, or, more precisely, the sketch of a version given beneath is based on his discussion in the Factorial analysis of human ability, 5th edn, 1961.

We are to suppose the mind to consist of a large number of 'bonds', and that each test randomly samples a certain fraction of these. Let us call our ^{tests} $\text{test}_1 \text{ test}_2 \dots \text{test}_n$ and, ^{for} simplicity, take the four test-example.

Then if we let P_1, P_2, P_3, P_4 stand for these fractions, and N for the whole pool of bonds, then the number common to the first two tests' will most probably be $P_1 P_2 N$, and so on.

Now, in virtue of a theorem proved by Weldon, the correlation between tests will be
$$r_{12} = \frac{P_1 P_2 N}{\sqrt{(P_1 N \cdot P_2 N)}} = \sqrt{P_1 P_2}$$

Accordingly, in any tetrad in a table of test-score correlations, we have quantities like the following

	3	4
1	$\sqrt{P_1 P_3}$	$\sqrt{P_1 P_4}$
2	$\sqrt{P_2 P_3}$	$\sqrt{P_2 P_4}$

and, as Thomson pointed out, 'the tetrad difference is most probably

$$\sqrt{P_1 P_3 P_2 P_4} - \sqrt{P_1 P_4 P_2 P_3} = 0$$

This, of course is a sufficient condition for hierarchy in Spearman's sense

Now, the precise nature of the 'bonds' was left rather vague by Thomson, though he suggested that they might be seen as having an 'all or none' action like neurones. Though not a zealous hereditarian, Thomson insisted that his theory did not commit him to the view that all men were equal.

On the contrary, the sampling theory would consider men also to be samples, each man possessing some, but not all, both of the inherited and the acquired neural bonds which are the physical side of thought. Like the tests, some men are rich, others poor in these bonds. Some are richly endowed by heredity, some by opportunity and education; some by both and some by neither. The idea that men are samples of all that might be, and that any task samples the powers which an individual man possesses, does not for a moment carry with it the consequences asserted of equal correlations and a humdrum mediocrity among human kind.

- 1 See chapter 1.
- 2 See chapters 2 and 7.
- 3 This is a subjective assessment, made after discussions with biologists and psychologists.
- 4 For a brief but illuminating account of non-professional, late 19th. century psychology, see S. Hynes, The Edwardian turn of mind, London (1968). See chapter 5. See also L.S. Hearnshaw A short history of British psychology, London (1964). See chapter 2, 'Experimental and institutional beginnings'.
- 5 Ibid , 171
- 6 Op.cit. (note 4).
- 7 For a brief account of the life of Spearman, see the following: Hearnshaw, op.cit. (note 4), chapter 13. Spearman's autobiographical essay in ed. C. Murchison, A history of psychology in autobiography, London (1930), vol. 1, 299 - 334. Sir Godfrey Thomson, 'Charles Spearman, 1863 - 1945', Royal Society obituary notices, 5 (1945 - 48), 373 - 385.
- 8 For accounts of William MacDougall, see especially MacDougall's autobiographical essay in the Murchison volume, op.cit. (note 7), 191 - 224.
- 9 For accounts of Cyril Burt see L.S. Hearnshaw, C.L. Burt , London (1972). Also, ed. E.G. Boring, A history of psychology in autobiography, Worcester, Mass (1952), vol. 4, 53 - 74. See also
- 10 Brown, who, as we shall see, was an important figure in the spread of mathematical methodology, notably via his work The essentials of mental measurement, Cambridge (1911). He was a student of MacDougall, working as reader at Kings College London from 1903. He was an early British student of Freud, writing on 'Freud's theory of dreams' in the Lancet vol. 19, 26 April 1913.
- 11 For details of Thomson, see Boring, op.cit. (note 9), 279 - 294.
- 12 For a discussion of Binet's life and work, see T. Wolfe, Alfred Binet Chicago (1973). For a general discussion of intelligence and mental testing in history, see Kimball Young, 'The history of mental testing', Pedagogical seminary, 31 (1923), 1 - 48.
- 13 See, for example, Spearman's claims in his autobiographical essay in ed. C. Murchison, A history of psychology in autobiography, London (1930), vol. 1, 299 - 334.
- 14 See Hans Eysenck, The inequality of man , Fontana Books (Glasgow, 1975), 53.
- 15 Peter Urbach, 'Progress and degeneration in the I.Q. debate', British journal for the philosophy of science , 25, (1974), 99 - 135.
- 16 For an outline, see C. Spearman, The abilities of man : their nature and measurement, London (1927), chapters 1 to 5.
- 17 C. Spearman, '"General intelligence": objectively determined and measured', Am. jnl. psychol. , 15 (1904), 201 - 292.

- 18 Op.cit., (footnote 17), 284.
- 19 The idea of 'saturation' with 'g' is developed clearly in Godfrey Thomson's Factorial analysis of human ability, 5th. edn. London (1951), 9.
- 20 In Spearman's perspective, of course, intelligence tests became tests of 'g', or, at least, well - constituted ones did.
- 21 Spearman's methods were developed in Spearman op.cit. (footnote 13), and are also discussed in W.Brown and G.Thomson, Essentials of mental measurement, Cambridge (1921). See chapter 8 in particular.
- 22 For the derivation, see C.Spearman and Bernard Hart, 'General ability. Its existence and nature', British journal of psychology, 5 (1912), 51 - 84. See especially p.58.
- 23 For details, see Helen Walker, Studies in the history of statistical method, Baltimore (1931). See chapter 6 'The theory of two factors'.
- 24 Op.cit. (note 17). See chapter 20, 'The sampling theory'
- 25 For a listing of Spearman's students, see Murchison, op.cit. (note 7), 326.
- 26 The introduction of 'g' in the 1904 paper does not come, initially, via the hierarchy, but via the application of a specialised theorem in correlation, with metaphysical interpretation superadded, but first developed in Spearman's paper 'Proof and measurement of association between two things', Am.jnl.psychology., 15 (1904), 72 -101. The theorem is discussed in Brown and Thomson, op.cit. (footnote 21). This fact alone seems to throw doubt upon Thomson's claim in his discussion of Spearman (Royal Society obituary notices, 5 (1945 - 48), 373 - 385) that Spearman's work sprang from his noticing a hierarchy in his collected data. The point is reinforced by Burt's observation that the famous Spearman hierarchy (Table 1) is a hierarchy of 'corrected' coefficients, and that the uncorrected coefficients make an observed hierarchy unlikely. (See, Cyril Burt, 'The two-factor theory', Brit.jnl.stat. psychology, 2 (1949), 151 - 179. See, for this point, p. 165). Burt also argues, p. 152, that Spearman's 'proof' of his two-factor theory 'therefore, is to be considered as an inductive corroboration of a hypothesis already considered highly probable on a priori grounds'. This, certainly, is the view advanced in this chapter.
- 27 See Murchison, op.cit. (note 7)
- 28 See, for example, Spearman's work, The nature of 'intelligence' and the principles of cognition, London (1923). See chapters 1 to 7 inclusive.
- 29 L.S.Hearnshaw, op.cit. (note 4), 200
- 30 See, for example, Passmore, A hundred years of philosophy, Penguin books (1972). See chapter 4 'Personality and the absolute'. See also D.W.Hamlyn, 'Bradley, Ward and Stout', in ed. B.Wolman, Historical roots of contemporary psychology, London (1968). For discussion of Waid, see F.M.Turner, Between science and religion, London (1974). See chapter 8.

- 31 For an account of Kant's influence, see D.B.Klein, A history of scientific psychology, London (1970). See chapter 15, 'The Kantian background'.
- 32 William MacDougall, The energies of men, 3rd. edn, London (1935), 20
- 33 For an account of the principles of noegenetic synthesis, see Spearman, op.cit. (note 28). And, for Spearman's identification of 'g' with the power of noegenetic synthesis, see Murchison, op.cit. (note 7), 326.
- 34 See e.g., Hart and Spearman, op.cit. (note 22), 79.
- 35 C.Spearman, The abilities of man: their nature and measurement, London (1927), 407.
- 36 Ibid. 408.
- 37 For a discussion of this matter, see R.E.Fancher, Psychoanalytic psychology : The development of Freud's thought, New York (1973). See chapter 3, 'First model of the mind: "Project for a scientific psychology"'.
- 38 This is discussed in Spearman, op.cit. (footnote 12), 129 - 132.
- 39 W.MacDougall, 'The physiological factors of the attention process', Mind, xi (1902), 316 - 351. See especially p. 329, also the terminal note on 'neurin'.
- 40 W.MacDougall, Body and mind , London (1911) 282.
- 41 See e.g., G.Thomson's book, op.cit. (note 19).
- 42 For a discussion of these points, see chapters 2 and 7 of this thesis
- 43 Information gathered in conversation with Raymond E.Fancher, January 1977.
- 44 Darwin had hard views, as related in Gwen Raverat, Period piece: a Cambridge childhood, London (1952), 199.
- 45 See W.C.D. and C.D.Whetham, The family and the nation, London (19)
- 46 C.Spearman, 'The heredity of abilities', Eugenics review, 6, (1914 -15), 219 - 237 and 595 - 606.
- 47 C.Spearman and B.Hart, op.cit. (note 19), 78 - 79.
- 48 Spearman, op.cit. (note 46)
- 49 C.Spearman and B.Hart, op.cit. (note 22), 78 -79.
- 50 H.Walker, op.cit. (note 23).
- 51 'Popperian' in this context, means consistent with Karl Popper's ideas as outlined in his Logic of scientific discovery, London (1965).
- 52 Spearman, op.cit. (note 48).

- 54 In a letter to Oliver Gillie of November 9, 1976, I would like to thank Professor Clarke of the Department of psychology, University of Hull for permission to see copies of this correspondence.
- 55 L.Kamin, The science and politics of I.Q., Penguin books (1977).
- 56 L.S.Hearnshaw, op.cit. (note 9), 19
- 57 See Boring, op.cit. (note 9), 60
- 58 See L.S.Hearnshaw, 'Sherrington, Burt and the beginnings of psychology in Liverpool', Bull. Br. Psych. Soc., 27 (1974), 9 - 14.
- 59 C.L.Burt, 'Experimental tests of general intelligence', Brit.jnl. psychology, 3 (1909), 94 - 177.
- 60 Ibid, 99 - 100.
- 61 Ibid. 104.
- 62 For an account, see G.U.Yule and M.G.Kendall, An introduction to the theory of statistics, 14th.edn., London (1950), 262.
- 63 Op.cit. (note 59), 157.
- 64 Ibid., 176.
- 65 W.MacDougall, National welfare and national decay, London (1921), vii.
- 66 Ibid.
- 67 C.L.Burt, Intelligence and fertility (Eugenics Society), London (1952),
- 68 C.L.Burt, 'The trend of national intelligence', Brit.jnl.sociol., 1, (1950), 154 - 168
- 69 MacDougall's eugenic views were also paraded in W, MacDougall, 'Psychology in the service of eugenics', Eugenics review, 5 (1913), 295 - 308.
- 70 G.U.Yule, review of Brown and Thomson, Brit.jnl. psychology, 12 (1921), 100 - 107.

Chapter 9. Conclusions: problems in historical explanation.

We saw in the second chapter that Galton made innovations in several areas. He invented eugenics, studied the inheritance of continuously varying characters, made strides in the development of statistical theory, speculated on intelligence and pioneered mental testing. For him, all of these activities were facets of a single unified programme of social reform and scientific innovation.

It is not too much to claim that Galton set in train a distinctive 'Galtonian tradition', simply because men of the renown and influence of Karl Pearson, Ronald Fisher and Cyril Burt, who developed particular aspects of Galton's thought were also committed to similar unified programmes of scientific innovation and social reform. On the other hand, the identity of concern between these men and Galton must not be overplayed, for, while Galton, Pearson and Fisher were all keen eugenicists for example, their social ideals varied considerably nonetheless. One (Galton) had an utopian ideal, a second (Pearson) was a state socialist, and a third (Fisher) wanted a better deal for the professional classes within traditional society. Pearson denounced the established church, Fisher was punctilious in matters of chapel attendance. Perhaps then, it is best to say that there was a Galtonian tradition in the sense that these and other intellectual leaders worked on programmes showing family resemblances.

Surrounding and interacting with these leading figures, of whom, in our chosen period, none outranks Pearson, were several followers pursuing the same line as their superiors. David Heron, perhaps, is the best example of these. At a stage removed is another more interesting fraction of scientists, composed of men like W.S.Gossett and G.U.Yule who engaged with the scientific work of the Galtonians but without sharing any strong allegiance to the social imperatives of the Galtonian tradition. Yule worked in statistics and genetics, but with little obvious commitment to the eugenic philosophy that guided the career of his superior Pearson. Consequently, intellectually and geographically, they separated. Much the same may be said of W.S.Gossett, whose work in statistics, as noted, was a response to the problems of the brewing industry rather than to the difficulties of evolutionary biology or eugenics. Others fitting into this third category of Galtonian, though perhaps more loosely, are Weldon himself and Spearman. Weldon seems to have cared little for eugenics, but was a Galtonian in the sense that he used and developed Galton's statistical methods when advancing his biological work.

Spearman had some eugenic allegiances and refined Galton's notion of energy (though, of course, the notion was hardly specific to Galton) but, again, we have no reason to suppose that the social imperative was a dominating force in his work. It does seem however that things were otherwise in the case of Cyril Burt.

With the passage of time the loosening of the tradition increased, to the extent that, at the present time, professional statisticians have no commitment to eugenics at all - though, of course, a compacted but 'integrated' Galtonian tradition still persists in the form of psychologists given to statistical methods and Fisherian genetics inquiring into the possible biological bases of class and race-differences in I.Q.. Thus they sometimes continue, in effect, Galton's 1869 inquiry into the 'comparative worth of different races'. Sometimes these investigations are surrounded by a rather Galtonian rhetoric, sometimes not.

In all events, we may surely agree that whatever be the utility of talking about 'traditions' in general, and about the 'Galtonian tradition' in particular, the followers of Galton - whether whole-hearted, half engaging, or 'passing through' - were a most influential and important set of men: collectively they were most fecund in the development of sciences which have had a considerable social and cultural impact. Of none is this more true than Pearson, who, as we have seen, made massive contribution to statistics, intellectually and institutionally, who provoked Fisher's important forays into genetics, and who provided differential psychologists with their statistical methodology.

It remains to inquire whether the knowledge of the scientific process acquired in this study leads to any general conclusions. Much as Darwin inquired whether there were not true general statements which might be made about the countless organic changes involved in the history of life, so one wonders whether some true general statement might not emerge here too.

In my view, it does not. But, there is at least one observation of general applicability which seems contrary to received wisdom (a loose phrase admittedly) about the scientific process. It is that, in one way and another, different sorts of non-empirical considerations appear to have played a crucial role in the development of science. We have seen, amongst

other examples, Galton and his utopian quest; Pearson's dedication to a Machian philosophy of science and to a form of state socialism built about a reformed system of ethics; Bateson's anti-materialism and anti-utilitarianism; Fisher's eugenicism; MacDougall's and Burt's desire to repair faults in the fabric of eugenic propaganda, and Spearman's wish to counteract an ethically unattractive associationistic psychology. It is besides the point to insist that these scientists could have done these things with some different and 'more pure' motivation, for this, assuredly, is how things were.

The range of these non-empirical considerations is a large one. There is, for example, a world of difference between, say, MacDougall's desire to repair eugenic propaganda and Pearson's desire to establish a Machian philosophy of science. But, what they have in common is that they dictated to the scientist that which was desirable and interesting. They dictated which points came to be seen as problems and which solutions thereto would be the most desirable. There is no doubt, for example, that Cyril Burt sincerely wished to show that the preparatory school boys were innately superior to the elementary boys.

At various points I have attempted to discuss whether such values became dominant or not - e.g., whether, for example, Burt's desire got the better of him, in the sense of leading him on to assert conclusions with a degree of force that was unwarranted by the available evidence. I think it clear that, on several occasions, values did become dominant in the sense outlined, but will not pursue this matter here as the plausibility of such judgements depends upon making certain assumptions about the nature and possibility of confirmation in science, and this leads into a whole area of philosophical inquiry and controversy which it would be injudicious to open up at this juncture.

But if it be established, and I hope that it has been, that many crucial developments in the growth of the sciences discussed in this work hinged crucially upon the scientists concerned having committed themselves to various non-empirical propositions, we must ask at least two further questions - (1), how have these propositions been incorporated, if at all, in the science of the aforementioned scientists?; and, (2), how are we to explain these scientists' having acquired their various commitments to these non-empirical propositions?

An outstanding example of the first sort is Pearson and statistics. He, it is clear, created statistics as a mathematical methodology that would incorporate his Machian philosophy. But, when the statistical methods were applied by other scientific workers, as, for example by Spearman, they were made to function in an entirely different way. This seems to reinforce a point that sociologists of knowledge have not always stressed - namely that leaders and followers may be amazingly different in their interpretations of what they are doing. It may be the case, for example, that Kepler would not have discovered his laws unless he had been a neo-Platonist, but, it is quite possible at a later stage to adopt the laws without the neoplatonism. There is, it would seem, only a very thin sense in which the non-empirical propositions that motivate a piece of scientific work become incorporated into the very scientific concepts and theories to which they lead. If there is a counter-example to this principle among the works studied, then, perhaps, Spearman's notion of 'g' comes close to fitting the bill. For, it would seem, one can hardly allow the existence of such a factor without allowing for the possibility of a linear ordering of men in terms of something like 'overall ability'. On the other hand, one does not, unlike Spearman, have to see this as a desirable or even as an interesting result.

The second question is the more serious, as it brings us straight to a major problem in the explanation of scientific change - namely that of whether we can explain the scientists in question holding to their respective non-empirical commitments by looking to, say, the social structures of their times. The significance of the question is obvious. For, were it possible to explain scientific developments thus in some very strong sense, then the often - assumed hierarchy of the sciences would be overthrown. Biology and psychology would be reduced to sociology rather than vice-versa. The response I wish to give is not such a revolutionary one, but, rather to suggest that, in certain important cases at least, we can discern a significant manner in which advances in science may be viewed as dependent to some degree upon prevailing social structures. As before, let us consider the case of Karl Pearson.

Now, Pearson's non-empirical views, which, we have seen, guided his scientific course, may be seen as harmonising with the natural interests (or, at least, with one possible set of natural interests) of the professional middle class of which he was a member. There is a sense, Hobsbawm has shown,

in which, in the late 19th century, professional middle class persons were able to draw up a social blueprint in which they would play a significant and esteemed role - that presented to them by Fabianism. Such persons were growing in number, did not derive incomes from land or investments, and lived in economic conditions that sapped belief in laissez-faire. Certainly, Pearson's non-empirical views, as illustrated by his advocacy of eugenic state socialism dominated by those with scientific knowledge, harmonised with these Fabian interests. His views, then, were not totally aberrant; rather, they fitted in with and, doubtless were reinforced by, the views of other influential middle-class professionals. And, if this be the case, one may surely argue that Pearson's scientific work just cannot be understood in isolation from its social context. To this extent then, we might talk of the 'social determination' of science.

On the other hand, of course, this is a very feeble sense of 'determination'. Certainly, it does not add up to claims that either (i), Pearson's scientific interests were rigidly fixed by his social position, or (ii), that the eventual success or failure of his theories was dependent on social structure rather than upon empirical data. To maintain either (i) or (ii) would be impossible, for, on the one hand, not all of Pearson's social peers did the sort of work which he did, and, on the other, as we have seen, Pearson himself was obliged to admit that the data did not allow, for example, the universal application of the strong form of his interpretation of Galton's law of ancestral heredity.

Finally, and quite generally, this study may be of use when considering some of the issues which are currently receiving attention in the public prints - issues relating to the role of various non-empirical factors in science, notably, though not exclusively, in the context of current debates over Sociobiology. Sir Andrew Huxley, for example, in his recent (1977) address to the British Association, has spoken of the need for an absolute divorce between science and politics, and for an acknowledgement that there is a real and important distinction in science between 'actual evidence' for theories on the one hand, and 'more indirect inferences which are appropriate only for suggesting new approaches to the solution of the problem in hand'.

The issues raised by Sir Andrew are too complex to be tackled properly here, which, in any case, is an inappropriate locus, but we can at least see that, in important historical practice, the progress of science has

been due in part to persons of undisputed political and ideological persuasions, whose work was guided by these persuasions. We can also see that, as in the case of Bateson and genetics, the early survival of a new theory in the face of apparent anomalies has been due to its espouser's commitment to what Huxley would regard as 'broad unifying principles' as much as to 'actual evidence'. Clearly, there is still a great scope for discussions of the proper and appropriate roles for politics, ideology and metaphysical principles in science. It is hoped that the studies contained in this essay will serve as enlightening foci in such discussions.

NOTES

- 1 Sir Andrew Huxley, 'Evidence, clues and motives in science', reprinted the Times Higher Education Supplement; 2 September, 1977, 4 - 6.

Select Bibliography.

This work focuses on the works of a comparatively small number of highly gifted men, and on the conditions under which they laboured. I have included full references in all the notes, and the notes appended to each chapter give a good indication of works consulted whilst preparing the text. To this I now append a select bibliography consisting of (a), an alphabetical list of approximately 100 monographs of general relevance to the text, and (b), a list of prepared bibliographies of these men. The rationale for such a strategy will be appreciated when it is noted that Karl Pearson's official bibliography, for example, contains 648 items.

- G.Allen, Life science in the twentieth century, London (1975).
 D.Allen, The naturalist in Britain, London (1976).
 P.Abrams, The origins of British sociology; 1834 - 1914, Chicago, (1968).
 S.B.Barnes, Sociology of science, Penguin (1972)
 B.Bateson, William Bateson F.R.S., Cambridge (1928)
 G.Basalla, W.Coleman and R.Kargon, Victorian science, New York (1970).
 J.Ben - David, The scientist's role in society, New Jersey (1971)
 J.H. Bennett, Collected papers of R.A.Fisher, 5 vols. Adelaide (1971 -74)
 C.Bibby, T.H.Huxley, scientist, humanist and educator, London (1959).
 L.J.Bischoff, Intelligence: statistical concepts of its nature, New York (1954).
 A.L.Bowley, F.Y. Edgeworth's contributions to mathematical statistics,
 London (Royal Statistical Society) (1928).
 A.W.Brown, The metaphysical society: Victorian minds in crisis 1869 - 1880,
 London (1947).
 W.Brown and G.Thomson, The essentials of mental measurement, Cambridge (1921)
 R.D.O'Butler, The roots of national socialism, 1783 - 1933, London (1941).
 L.L.Cavalli Sforza and W.F.Bodmer, The genetics of human populations,
 San Francisco (1971).
 G.K.Chesterton, Eugenics and other evils, London (1922)
 W.Coleman, Biology in the nineteenth century. Problems of form, function
 and transformation, London (1971)
 R.Clark, J.B.S. The life and work of J.B.S.Haldane, London (1968).
 J.G.Crowther, British scientists of the twentieth century, London (1952)
 M. Cullen, The statistical movement in early Victorian Britain: the
 foundations of empirical social research, Sussex (1975).

- S.P.Davies, The mentally retarded in society, New York (1959)
- C.Darwin, The origin of species: a variorum text, ed. Morse Peckham Philadelphia (1959).
- C.Darwin, The descent of man, London
- R.Dawkins, The selfish gene, Oxford (1976)
- L.C.Dunn, A short history of genetics, New York (1965).
- W.P.Elderton, Frequency curves and correlation, London, 1927.
- H.J.Eysenck, The inequality of man, Glasgow (1775) - Fontana edn.
- D.K.Forrest, Francis Galton: The life and work of a Victorian genius London (1974).
- L.Farrall, The origin and growth of the English eugenics movement (Ph.D. thesis, Indiana University, Bloomington, 1970) - available from University microfilms
- R.Fancher, Psychoanalytic psychology. The development of Freud's thought, New York, 1973.
- D.Gasman, The scientific origins of national socialism, London (1971)
- J.K.Galbraith, New industrial state, Penguin books, (2nd edn) (1975).
- R.Giere and S. Westfall, Foundations of scientific method in the nineteenth century, Bloomington (1973).
- G.L.Geison, Sir Michael Foster and the rise of the Cambridge School of physiology, 1870 - 1900, (Unpublished Yale Ph.D. thesis, 1970) Available from University Microfilms.
- Elizabeth Gasking, The rise of experimental biology, New York (1970).
- I.Hacking, The emergence of probability, London (1975)
- I.Hacking, The logic of statistical inference, Cambridge (1965).
- J.S.Haller jnr., Outcasts from evolution, Chicago (1971).
- J.B.S.Haldane, Karl Pearson, 1857 - 1957, Biometrika Trustees, London (1958)
- M.Haller, Eugenics: Hereditarian attitudes in American thought, New Brunswick, (1973).
- L.S.Hearnshaw, A short history of British psychology, London (1964).
- G.Himmelfarb, Darwin and the Darwinian revolution, London (1968).
- E.J.Hobsbawm, Labouring men, Studies in the history of labour, London, (1968).
- E.J.Hobsbawm, Industry and empire, Penguin (1974).
- R.Hofstadter, Social Darwinism in American thought, Boston (1955)

- L.Hogben, Dangerous thoughts, London (1959).
- L.Hogben, Genetic principles in medicine and social science, London (1931).
- S.Hynes, The Edwardian turn of mind, London (1968).
- K.Jones, A history of the mental health services, London (1972).
- J.Jinks (ed.), Fifty years of genetics, Edinburgh (1969)
- C.J.Karier, The roots of crisis.American education in the twentieth century, Chicago (1973).
- L.Kamin, The science and politics of I.Q.,Penguin Books (1977).
- P.Keating, Into unknown England, London (1976)
- J.M.Keynes, Essays in biography, 3rd.edn.,London (1951).
- V.Kellogg,Darwinism today, New York, (1907).
- A.Koestler,The case of the midwife toad, London (1971)
- I.Lakatos and A.Musgrave, Criticism and the growth of knowledge,London, (1970).
- K.Ludmerer, Genetics and American society, Baltimore (1972).
- J.Maynard Smith,The theory of evolution, Penguin Books, 2nd.edn.(1966).
- J.Maynard Smith,On evolution, Edinburgh (1972).
- W.D.Mallock, The new republic, London (1877).
- M.Mandelbaum, History,man and reason.A study in nineteenth century thought, London (1971).
- K.Mannheim, Essays on sociology and social psychology, London (1953).
- J.T.Merz, A history of European thought in the nineteenth century, 4 vols. ,Edinburgh,(1907 - 1914).
- K.Martin,Harold Laski, A biographical memoir, London (1953).
- C.Murchison (ed.) A history of psychology in autobiography, 4 vols., London (1930 - 1952).
- T.McKeown,The modern rise of population, London (1976).
- R.Olby, Origins of Mendelism, London (1966).
- V.Passmore, A hundred years of philosophy,Penguin Books, (1972)
- N.Pastore, The nature-nurture controversy, New York (1949).
- J.D.Y.Peel, Herbert Spencer, the evolution of a sociologist, London (1971).
- H.Perkin, The origins of modern English society, 1780 - 1830, London, (1969).
- E.S.Pearson,Karl Pearson,An appreciation of some aspects of his life and work, Cambridge (1938).
- E.S.Pearson and M.G.Kendall (eds) Studies in the history of statistics and probability, London (1970).

- E.R.Pease, The history of the Fabian Society, London (1916).
- G.Raverat, Period piece .A Cambridge childhood, London (1952).
- C.E.Rosenberg, No other gods.On science and American social thought, Baltimore (1976).
- L.C.B.Seaman, Post-Victorian England, London (1967).
- C.W.Saleeby, Parenthood and race-culture ,London (1909).
- G.R.Searle, The quest for national efficiency.A study in British politics and British political thought,1889 - 1914, Oxford (1971).
- G.R.Searle, Eugenics and politics in Britain, 1900 - 1914, Leyden, (1976).
- C.U.M.Smith, The problem of life, London (1976).
- D.c. Somervell, English thought in the nineteenth century, 2nd. edn London (1929)
- B.Simon, Intelligence. psychology and education .London (1971)
- B.Semmel, Imperialism and social reform, London (1960).
- C.Singer, A short history of biology, London (19)
- A.H.Sturtevant, History of genetics.
- G.H.Thomson, The factorial analysis of human ability, 5th.edn, London (1951).
- D.Thomson, England in the nineteenth century, Penguin Books (1975).
- D.Thomson, England in the twentieth century, Penguin Books (1975).
- R.D'Arcy Thomson, D'Arcy Wentworth Thomson, The scholar naturalist, 1860 - 1948, with a postscript by P.B.Medawar, "D'Arcy Thomson and growth and form", Oxford (1950).
- R.Thomson, The Pelican history of psychology, Penguin Books, (1968).
- P.Thompson, The Edwardians.The remaking of British Society, London (1975).
- W.S.Smith, The London heretics,1870 - 1914, London (1967).
- R.D.Tuddenham, 'The nature and measurement of intelligence', in L.Postman (ed.) Psychology in the making, New York (1966).
- F.M.Turner, Between science and religio., New York (1974).
- A.F.Tredgold, Mental deficiency (Amentia), 2nd.edn, London (1914).
- H.Y.Vanderpool (ed) Darwin and Darwinism, London (1973).
- A.R.Wallace, My life: a record of events and opinions, London (1905), 2 vols.
- H.M.Walker, Studies in the history of statistical method, Baltimore (1931)
- B.Webb, My apprenticeship, Penguin Books edn. (1971)
- W.C.D.Whetham, Cambridge and elsewhere.The memoirs of Sir W.C.Dampier, formerly Whetham, London (1950).

- W.C.D. and C.D. Whetham, An introduction to eugenics, Cambridge (1912)
- T. Wolfe, Alfred Binet, Chicago (1973)
- B. Wolman, Historical roots of contemporary psychology, London (1968).
- R.M. Young, Mind, brain and adaptation in the 19th century, Oxford (1970)
- G.U. Yule, An introduction to the theory of statistics, London (1911)

Bibliographies and sources on major figures.

(1) Karl Pearson. The standard bibliography is G.M. Morant, A bibliography of the statistical and other writings of Karl Pearson, Cambridge (1939). For manuscript sources, consult the Pearson Archive, D.M.S. Watson Library, University College London: Copies of Maxine Merrington's Guide to the collection are available from University College, and the Archive is fully catalogued.

(2) Francis Galton. A comprehensive bibliography is contained in D.K. Forrest's book, Francis Galton, the life and work of a Victorian genius. For manuscript sources, consult The Galton Archive, University College London. The papers are fully catalogued.

(3) W.F.R. Weldon. A complete bibliography of Weldon's works is contained in Pearson's obituary of Weldon, in Biometrika, 5 (1906), 1 - 52. For manuscript sources, consult the Pearson Archive, University College London.

(4) William Bateson. Lists of Bateson's works are contained in B. Bateson, William Bateson, F.R.S., Cambridge (1928), and also in ed. R.C. Punnett, Scientific papers of William Bateson, 2 vols., Cambridge (1928). Some of Bateson's papers are in the Cambridge University Library, and others in the John Innes Institute. There is also a collection of 'Bateson papers' on microfilm at the Library of the American Philosophical Society, Philadelphia.

(5) R.A. Fisher. For details of Fisher's work, see ed. J.H. Bennett, Collected papers of P.A. Fisher, vols. Adelaide (1971) The first volume contains a complete bibliography.

(6) Charles Spearman. For a bibliography of Spearman, see Godfrey Thomson, 'Charles Spearman, 1863 - 1945', Obituary notices of Fellows of the Royal Society, 5 (1947), 373 - 385.

(7) Cyril Burt. A complete list of Sir Cyril Burt's publications has been prepared by Gillian Sutherland and Steve Sharp of Newnham College Cambridge. Copies are available on request. A biography is under preparation, in the hands of Professor L.S.Hearnshaw, Department of Psychology, University of Liverpool.

(8) G.U.Yule. See A.Stuart and M.G.Kendall (eds) The statistical papers of George Udny Yule, London, (1971).

(9) Galton and Biometric Laboratories. For a complete listing of the output of the Galton Eugenics Laboratory and of the Biometric Laboratory in the period to the end of the First War, see the bibliography attached to Lyndsay Farrall's unpublished doctoral thesis (Bloomington, Indiana, 1970). The origin and growth of the English Eugenics movement.

(10) W.K.Clifford. See in particular, L.Stephen and F.Pollock (eds), Lectures and essays by the late William Kingdom Clifford, 3rd.edn. 3 vols, London (1901).

(11) F.Y.Edgeworth. For a complete listing of statistical works, see A.L.Bowley, F.Y.Edgeworth's contributions to mathematical statistics, London (1928)